THE AMERICAN
JOURNAL OF PSYCHOLOGY

Founded by G. Stanley Hall in 1887

Vol. XXIV JULY, 1913 No. 3

THE RÔLE OF KINAESTHESIS IN THE PERCEPTION OF RHYTHM[1]

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I. Introduction

The experimental investigation of the perception of rhythm has grown so extensive and, at the same time, so indefinite in scope that the writing of an introduction which shall be adequate to the general problem is now altogether out of the question.[2] The subject of rhythm has been carried over into many fields both inside and outside of the science of psychology: within, it has been related to attention, work, fatigue, temporal estimation, affection, and melody; without, it is fre-

[1] From the Psychological Laboratory of Cornell University.

[2] We expect to publish a complete bibliography on the subject of *rhythm* in the next number of this *Journal*.

quently mentioned in connection with music, literature, biology, geology, gymnastics, physiology, and pedagogy. If we follow out its progress within the range of psychological investigation, we find, again, an intricate plexus of results, theories, and issues. Emphasis has been laid on various component factors of consciousness involved in the perception of rhythm: changes of affective processes; effect of pitch, of duration, of intensity of stimulus on rhythmic perception; types of possible rhythmic perception; part played by different modalities of imagery; bases for rhythmic grouping and accentuation. An adequate summary of the work done even in this limited field would fill a fairly large monograph.

It is now, furthermore, no longer possible to undertake a set of experimental investigations, as was done twenty years ago,[3] to cover the entire range of the perception of rhythm. Attacks directed at the problem from various points of view, and with various methods of procedure, are the run of the day. No one, as far as the literature tells, has since that time attempted to make a complete study of rhythm.

A. Historical.—There seems to be one factor, however, which, when singled out and brought into perspective with other factors, has been assigned a prominent place in the discussion of rhythm. It would not be permissible to say that this factor has itself been the subject of discussion, for, curiously enough, it has often been taken for granted,—regarded, as it were, from an established point of view. Kinaesthesis of one sort or another, or motor expression consciously represented in the form of imagery or perceptual complex, is regarded by most investigators in this field as essential to rhythmic grouping and accentuation; it is considered, by a few, as non-basic but contributory and perhaps, indeed, as jointly essential; it is not mentioned at all by a few others. The point is, however, that the last class of investigators do not enter the lists polemically against those who emphasize the presence of the kinaesthetic component in the perception of rhythm. They simply advance a theory which turns out to be different.

Meumann comes to the conclusion that rhythms are perceived as the result of a mental activity conditioned either by the temporal quality of the perception or by the unitariness of meaning logically superimposed upon the impressions perceived. He says:

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[3] *Cf.* Bolton, T. L. Rhythm, this *Journal*, 6, 1894, 145-238.

In every case of rhythmical perception we group the isolated sensations of sound into a more or less complex system of ideas that are temporally arranged.[4]

For some observers the combining of the impressions is at the same time a temporal concatenation in the sense that the members of a group appear to follow one another more quickly, while a pause is experienced between every two groups.[5]

The other form of grouping is typified by the following quotations:

This process of grouping was recalled and described to me by my observers as an *'inner combination';* the single beats were taken out of their incipient isolation and appeared as parts of a whole; the groups, on the other hand, were clearly separated one from another.[6]

The peculiarity of the whole experience of rhythm, it seems to me, is its *pure central initiation.* Either the reproduction of familiar meters, or better, perhaps, the *unequal distribution of the energy of attention* must be considered in this connection.[7]

But the course of 'thoughts' becomes itself the condition of rhythmisation. The character of the entire rhythm is preëminently determined by this delimitation of meaning. The groups which appear collectively to the ear (and, in this sense, the rhythmical units) are constituted as a kind of rhythmical treatment, which is peculiarly characteristic of Goethe's lyric verse.[8]

Woodrow, in closing the summary of his experimental work, also emphasises the temporal aspect of rhythmical grouping:

This close correspondence between the rhythmical grouping and the temporal grouping, or rather this correspondence in the points where both disappear, indicates that rhythmical grouping is a temporal grouping; that is, that rhythmical grouping is determined by the duration of the subjective intervals, not by the objectively measurable intervals, but by the subject's consciousness of these intervals, that is, by the intervals considered as mental magnitudes.[9]

Continuing to consider these classes of authorities in an inverse order, we find a group who conclude that kinaesthesis is a factor which is jointly essential to the perception of rhythm. Wundt, for example, gives a genetic explanation for rhythm in terms of the movements of locomotion, and then makes the essential characteristic of rhythmical perceptions dependent upon the changes in the course of affection, with a motor tendency in the background of consciousness:

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[4] Meumann, E. Untersuchungen zur Psychologie u. Aesthetik d. Rhythmus, *Philos. Stud.*, 10, 1894, 283.
[5] *Ibid.*, 304.
[6] *Ibid.*, 303.
[7] *Ibid.*, 304.
[8] *Ibid.*, 396.
[9] Woodrow, H. A quantitative study of rhythm, New York, 1909, 66.

So we have every reason to consider the movements of locomotion as the natural origin of rhythmical perceptions.[10]

Consciousness is rhythmically disposed, because the whole organism is rhythmically disposed. The movements of the heart, of breathing, of walking, take place rhythmically. . . . Above all, the movements of walking form a very clear and recognisable background to our consciousness.[11]

Pleasure and displeasure constitute, as we saw above, subordinate forms of rhythmic affections which, because they parallel in a relative equivalence the peculiar contents of pleasure and displeasure, we can designate as *formal* components of the aesthetic effect of feelings. In contrast to this, the characteristic *contents* of rhythmical effect results from the *specific form of rhythmical movement* which is, in turn, capable of very numerous gradations and in which varying mixtures of different components of affection and changes between qualitatively differing and, indeed, opposed affections, are possible. Because, at the same time, this contents always partakes of a certain affective character, we can contrast with it that formal or abstract affective quality of pleasure and displeasure under the concept of the *affective* components of rhythm.[12]

In essentials Stumpf does not disagree with Wundt in this notion of the factors which are important in rhythm:

Judgments of time and intensity are further connected in the comprehension of rhythm; this we shall best discuss, however, in connection with the theory of affection.[13]

It looks, indeed, as if our sense of rhythm and time was essentially developed in connection with the movements of locomotion.[14]

Ebhardt assigns to a number of factors the responsibility of giving rise to rhythmical phenomena, but lays most stress on the importance of affective tone:

The following assumptions were made as hypothetical explanations of these phenomena: peculiarities of motor action, change of direction of attention, and the combination of members into groups or the separation of groups in the process of comprehension and in the process of execution corresponding to it.[15]

This affective tone, it appears to me, forms an essential component of rhythm; it must be present if there is not to be a failure to comprehend the rhythm: the player would miss something if he could not successfully arouse and maintain an affective contents.[16]

But by far the greater number of investigators and systematic writers on the subject of rhythm emphasise the primary

[10] Wundt, W. Grundzüge d. physiologischen Psychologie, 6th ed., 1911, vol. III, 32.

[11] Wundt, W. Introduction to psychology (trans. Pintner), New York, 1912, 5.

[12] Grundzüge, vol. III, 147.

[13] Stumpf, C. Tonpsychologie, Leipzig, 1883, vol. I, 135.

[14] *Ibid.*, 340.

[15] Ebhardt, K. Zwei Beiträge zur Psychologie d. Rhythmus u. d. Tempo, *Zeits. f. Psychol.*, 18, 1898, 127.

[16] *Ibid.*, 140.

importance of kinaesthesis and of motor response in rhythmical perceptions. Whether this motor response is consciously represented, a few of these writers do not specify, but the majority use the phrase as an equivalent for kinaesthesis. Taken up in historical order, we find Bolton, the pioneer American investigator, making the statement:

> We come now to the subject of muscular movements and their relation to rhythm. Most subjects felt themselves impelled by an irresistible force to make muscular movements of some sort accompanying the rhythms. If they attempted to restrain these movements in one muscle, they were likely to appear somewhere else. . . . The question we have to decide is, are these muscular movements and associations the result or the conditions of the rhythmical grouping? With Ribot we accept without hesitation the latter. . . . Each impression as it enters into consciousness tends to find expression in a muscular movement, but the intensive changes in the series of impressions produce corresponding changes in the intensity of the sensations, which must find expression in different degrees of muscular activity. In order to express these different degrees of sensation, the muscular movements must rise above the merely nascent state in which they ordinarily occur, and manifest themselves in visible muscular movements.[17]

Ettlinger, while working and theorising more about the aesthetical aspect of rhythm, says:

> The descending grouping of two elements [trochee] represents, therefore, the transition from a period of rest to a period of advance, the initiation of a movement. If such a group stands at the beginning of a series, then it indicates with especial clearness the origin of a movement. Outside of this, inasmuch as there is no return to a period of rest to be found in it, it points on this account to a perpetuation of the movement. When, therefore, it appears at the end of a movement, it does not bring that movement definitely to completion, but allows it to be dissipated in empty space.[18]

MacDougall accounts for the temporal element in rhythmical perception on the basis of organic activity and of kinaesthesis:

> The fundamental conditions of the rhythm experience are therefore to be looked for in the laws of periodicity of functioning in the bodily organism. It is because these processes take place under conditions of regularly recurrent change that the time element becomes important in rhythmical sequences.[19]

Elsewhere, in much the same manner, he describes the effect of mechanical conditions imposed on the expression of rhythm by processes of muscular contraction and release.[20]

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[17] *Op. cit.*, 234.

[18] Ettlinger, M. Zur Grundlegung einer Aesthetik d. Rhythmus, *Zeits. f. Psychol.*, 22, 1900, 186-7.

[19] MacDougall, R. The relation of auditory rhythm to nervous discharge, *Psychol. Rev.*, 9, 1902, 465.

[20] The structure of simple rhythm forms, *Monog. Suppl., Psychol. Rev.*, 4, 1903, 327.

Speaking of kinaesthesis, Miner, who has made a special study of motor rhythms, concludes:

> I believe it is time to recognise that it is this element in the conscious experience which adequately affords the explanation of the main fact of rhythm,—the group feeling. To be sure we do not stop here. There are many other attributes of the rhythmic impression to be accounted for. . . . The advantage of the kinaesthetic thesis which is here offered is that it gives a satisfactory mode of approach for these other problems.[21]

Stetson, under the heading, *Suggestions for a motor theory of rhythm*, opens a discussion of the motor theory as applied to rhythmical perceptions with the words:

> If the basis of rhythm is to be found in muscular sensations, rather than in the supposed activity of some special 'mental' function, the nature of the movement cycle involved is of greatest interest.[22]

And he concludes:

> Assuming such a movement cycle, in which the tension between the two opposing sets never comes to zero until the close of the series, it is not difficult to arrange many of the facts of rhythmic perception under the motor theory.[23]

One of the six conclusions, drawn by Koffka as the result of experimental investigations, reads:

> Motor imagery has, however, the greatest significance for rhythmical experiences. It appears almost universally, and is, for the most part, very difficult to suppress.[24]

Agreeing, in the main, with Stetson, Bingham assumes a motor theory for both rhythm and melody:

> The experimental study of rhythm has, however, disclosed a motor phenomenon essentially like the large, basic motor activity underlying a melodic unity.[25]

Krueger can also be classed in this group:

> The affectively toned auditory experiences, especially the rhythmical, never appear without the characteristic sensory components of motor origin.[26]

There are several systematic writers who have given the kin-

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[21] Miner, J. B. Motor, visual, and applied rhythms, *Monog. Suppl., Psychol. Rev.*, 5, 1903, 17.

[22] Stetson, R. H. Rhythm and rhyme, *Monog. Suppl., Psychol. Rev.*, 4, 1903, 453ff.

[23] Koffka, K. Experimental-Untersuchungen zur Lehre vom Rhythmus, *Zeits. f. Psychol.*, 52, 1909, 104.

[24] Bingham, W. V. D. Studies in melody, *Monog. Suppl., Psychol. Rev.*, 12, 1910, 83.

[25] Krueger, F. Mitbewegungen beim Singen, Sprechen, u. Hören, Leipzig, 1910, 22.

aesthetic theory place in textbooks of psychology. Among these Titchener may be mentioned:

> The author was formerly disposed to attribute a separate rhythmical perception to hearing, but recent observation has convinced him of the existence of kinaesthetic sensations due to the contraction of the *tensor tympani* of the middle ear. . . . In the author's opinion, this [visual] rhythm is always kinaesthetic, based upon eye-movement, upon slight movements which tick off the successive impressions, or upon some other form of intermittent kinaesthesis.[26]

Wallin, one of the most recent investigators in this field, says:

> Practically all the subjects made use of kinaesthetic factors—movements of the body, or tongue, or head, or finger.[27]

And Swindle, who gives us the latest study, remarks:

> In the development of a rhythm, the motor activity of the skeletal muscles plays the most important rôle.[28]

There is evidence enough, then, that most of the investigators in the field of rhythm conclude that kinaesthesis of one sort or another plays the most prominent part in rhythmical perception and in its development. This is the factual side of the case: with possibly only a few exceptions, all the statements made are the result of observations under controlled conditions.

B. Theoretical.—A large literature has recently made its appearance in connection with discussions concerning the fundamental principles of psychology. With it has come the slogan of 'the science of human behavior.' This plea for the study of behavior brings with it, on the theoretical side, a heavy stress on the motor responses of the organism. Statements like the following, selected at random from the works of expositors of this type of psychology, illustrate the tendency toward a renewed emphasis on kinaesthesis:

> The science is being developed on the one hand by zoölogists and on the other hand by comparative psychologists. These scientists are studying those visible movements of the animal organism which constitute the external physiological processes.[29]
>
> We may express the relation which actually obtains between them by saying that physiology investigates the processes of the parts or organs of which any organism is composed, while psychology investi-

[26] Titchener, E. B. A text-book of psychology, New York, 1910, 345.
[27] Wallin, J. E. W. Experimental studies of rhythm and time, *Psychol. Rev.*, 19, 1912, 295.
[28] Swindle, P. F. On the inheritance of rhythm, this *Journal*, 24, 1913, 202.
[29] Parmelee, M. The science of human behavior, New York, 1913, 1.

gates the activities of the organism as a whole, that is those in which it operates as a whole or unit.[80]

If psychology is to be defined as the science of human behavior, the term 'behavior' must be used in the widest sense. It must include everything from the simplest movements of walking or of fingering the pen to the activities involved in swaying an audience by speech or in carrying to completion some great engineering work.[81]

A complete explanation of any phase of consciousness can be neither in terms of sensation nor in terms of movement exclusively, but must include both.[82]

The scheme of habit which James long ago described—where each return or afferent current releases the next appropriate motor discharge—is as true for 'thought-processes' as for overt muscular acts.[83]

The psychology which I should attempt to build up would take as a starting point, first, the observable fact that organisms, man and animal alike, do adjust themselves to their environment by means of hereditary and habit equipments. These adjustments may be very adequate or they may be so inadequate that the organism barely maintains its existence; secondly that certain stimuli lead the organisms to make the responses.[84]

In theory at least, if not in substance, all of these references hark back to the well-known and oft-quoted passage from Ribot:

It is always necessary to recall that fundamental principle: every mental state is accompanied by manifestations which are physically determined. Thought is not—although many, influenced by tradition, would admit it to be—an event that has passed into a suprasensible, ethereal and incomprehensible world. We repeat with Setchenoff, 'no thought without expression,' *i.e.*, thought is a word or an act in a nascent state, that is to say, the beginning of a muscular activity.[85]

But already has this movement to emphasise the motor response as an interpretable datum of psychology found antagonists. Thorndike assails the 'ideo-motor' theory in part with these words:

The theory of ideo-motor action has been for a generation one of the stock 'laws' of orthodox psychology. It is taught as almost axiomatic in standard treatises—is made the explanatory principle for phenomena of suggestion, hypnotism, obsessions and the like—and is used as the basis for recommended practices in education, psychiatry, religion—even in salesmanship and advertising. . . .

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[80] McDougall, W. Psychology, the study of behavior, New York and London, 1912, 35.

[81] Pillsbury, W. B. The essentials of psychology, New York, 1911, 2-3.

[82] Pillsbury, W. B. The place of movement in consciousness, *Psychol. Rev.*, 18, 1911, 99.

[83] Watson, J. B. Psychology and behavior, *Psychol. Rev.*, 20, 1913, 174 (note).

[84] *Ibid.*, 167.

[85] Ribot, Psychologie de l'attention, Paris, 1889, 20.

The connection whereby likeness or representative quality, in and of itself, created a bond between a thought and an act, would be 'mysterious' if it existed. But it does not exist.[36]

If the motor response is to be taken *ipso facto* as a criterion to which we are to refer the contents of consciousness, with the additional strong subsumption that it is the only criterion, then it appears that the elaborative functions of the cortex are summarily ruled out of court.[37] That under varying conditions we consciously ascribe divers meanings and references to our kinaesthetic complexes, and that kinaesthesis, aside from other contents of consciousness, is only a part of the material which the cortex elaborates, is apparently overlooked. As to the direct interpretation of 'movement-curves' as graphic or even symbolic representations of conscious processes, it seems only necessary to indicate that there is a manifest confusion of the categories of at least two sciences, psychology and physics. Even casual introspection of our everyday experiences shows that an extensive or an intensive motor response may count for very little in consciousness at one time, while an imperceptible movement or a kinaesthetic image may mean much at another. Psychological meaning is not a matter of extent or intensity of movement, but of extent of elaboration.[38] The spatial and temporal attributes of the physical universe cannot be taken over bodily into the psychological.

A psychophysical interpretation of the rhythmical consciousness can also be criticised from approximately the same angle. The assumption of an equivalence between the psychical and the physical is an error into which the investigator is apt to fall if he insists upon too rigid an interpretation of psychophysical data, or if he handles those data as if they exhausted the contents of consciousness under examination. Quantitative psychology must be made coördinate with qualita-

[36] Thorndike, E. L. Ideo-motor action, *Psychol. Rev.*, 20, 1913, 91-106.

[37] *V.* Titchener, E. B. Psychology of feeling and attention, New York, 1908, 309; also Bentley, M., this *Journal*, 17, 1906, 293f.

[38] Ebbinghaus, H. Grundzüge d. Psychologie, Leipzig, 1908, II § 70.
Höffding, H. Outlines of psychology, Leipzig, 1893, 114.
James W. Principles of psychology, New York, 1890, II, 3.
Ladd, G. T. Psychology, descriptive and explanatory, New York, 1894, 661.
Thorndike, E. L. Elements of psychology, New York, 1905, 65.
Titchener, E. B. A text-book of psychology, New York, 1910, 367.

tive psychology; neither is sufficient without the other.[39] So, while we cordially welcome psychophysical experimentation in this special field when its results are duly qualified in the manner indicated, we do not think that Woodrow's characterisation of the method of procedure and of the problem is at all comprehensive:

> The typical procedure in the investigation of these rhythmical factors has been to present a series of sounds or other stimuli, some of which are longer, more intense, or of different quality than the others, and to ask the observer to describe his impression of the series, especially as regards the way in which the stimuli appear to be grouped. Such a procedure is characteristically a psychophysical one, just as much as when two weights are presented to an observer for him to lift, and then to describe his impression of them with special reference to their relative heaviness. In both cases the investigation is one of the relationship between certain conditions existing in the environment and the nature of the subject's consciousness under the existing conditions.[40]

All this is but part of the problem. Not only must we investigate the "relationship between certain conditions . . . . . and the nature of the subject's consciousness," but, in order to establish the relationship between conditions that are known, *i. e.*, the physical conditions of the experiment, and the "nature of the subject's consciousness" which is unknown, we must investigate the nature of that consciousness. The history of the reaction experiment tells the same story: after decades of psychophysics, it was left for Ach to publish in 1905 his book *Ueber die Willenstätigkeit und das Denken*, in which for the first time a systematic psychology of volition and action, based upon introspective data from the reaction experiment, is presented.[41]

Although several investigators have contributed introspective material in the field of rhythm, there is still a dearth of analytical observations systematically controlled and obtained from trained and practised subjects. So the statement which Meumann made in 1894 still holds, in a measure, to-day:

> What we still lack, more than anything else, is a comprehensive description of the psychical phenomena of the inner life, which we call rhythmical, and a reference of these phenomena to the action of general psychical factors, as well as the investigation of the conditions which make rhythm possible.[42]

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[39] *Cf.* Titchener, E. B. Experimental psychology, New York, 1905, vol. II, pt. I, xxxviii.

[40] Woodrow, H. The rôle of pitch in rhythm, *Psychol. Rev.*, 18, 1911, 54.

[41] *V.* Titchener, E. B. This *Journal*, 21, 1910, 416.

[42] *Op. cit.*, 273.

II. Experimental Procedure.

It was partly in view of the fact that introspective material of an analytical sort is comparatively scarce, partly because we believed that in previous investigations not enough emphasis had been laid on that aspect of kinaesthesis which gave it *meaning* or *reference,* and partly on account of theoretical considerations in regard to the importance of contributing factual data which might help to strengthen or weaken current views centering about the motor responses of organisms, that this investigation was undertaken. The nature of the rhythmical consciousness had not been sufficiently analysed from the introspective side to warrant generalisation about its contents.

A. Problem.—The task before us was to produce by physical means a series of impressions which would, under ordinary conditions, group themselves together in a rhythmical relation; to secure as faithful a description of consciousness, under these conditions, as practice and training on the side of the observers would permit; and to use as effective means of control and check on conscious contents as methodological approach and systematic observation on the side of the experimenter would allow. Further analysed, our problem involved a variation of conditions in order to present rhythms of different kinds under objective control, and a variation of instructions for the purpose of bringing different types of consciousnesses under subjective control. Some of these instructions focused attention on the kinaesthetic complexes present, but these instructions were given only after there was definite evidence of actual kinaesthesis.

B. Observers.—The observers in this experiment were either members of the Department of Psychology or graduate students of one or more years' standing. They were: Dr. Geissler (Ge), instructor, Mr. Foster (F), research assistant, Mr. Boring (B), assistant in the department; Miss Burr (Bu), Miss Day (D), Mr. Edwards (E), and Miss Goudge (G), all graduate students in psychology. In a few observations Dr. Bentley (Be), assistant professor, and the writer (R), instructor in the department, also took part. None of the observers reported in all of the experiments; D and G served, however, in about three-fourths of the total number. Arranged in descending order, according to the amount of introspective training and practice, the observers may be classed as follows: Be; Ge; F; D; B, E, G, and R; Bu.

In order to ascertain the degree of musical training which the observers had, as well as to determine the approximate nature of their experiences in every-day life when they were listening to musical renditions, those observers who took part in the more important experiments were requested to answer the following confidential questionary:

1. Have you had any courses in any branch of music in high school or in college? If so, outline them.
2. Have you had any training in singing outside of school and college courses? If so, to what extent?
3. Do you play any musical instrument? If so, with what degree of proficiency? For how many years have you played?
4. Can you tell the approximate absolute pitch of any given note?
5. Can you analyse any given chord into its constituent parts? Can you name the given chord?
6. Do you consider yourself possessed of a good 'musical ear'?
7. To what extent, concisely stated, do you appreciate music?
8. Psychologically considered, how does music generally affect you —what phenomena does it arouse?
9. Do you carry melodies auditorily? If not, how do you carry them?
10. To what extent do melodies play a part in your every-day consciousness?

(After you have answered the above, uncover and answer question No. 11 below this slip.)

11. UNDER THIS INSTRUCTION RECALL A CONCERT OR RECITAL WHICH YOU HAVE RECENTLY ATTENDED: What have you now in consciousness?

Although most of the observers stated that they would be willing to have the answers to these questions published, the writer considered it fairer to treat the information obtained as confidential. While, therefore, the records of the questionary are available in the protocol, we must rest content with generalisations based upon the answers received without making their application personal. Almost all of the observers had had practice in playing some musical instrument with some degree of proficiency; over one-half of the number had received instruction in piano-playing, organ-playing, violin-playing, or singing, covering periods from about one year to five years or more in duration. A few had learned the rules of writing harmony and melody; one observer possessed the sense of absolute pitch. Almost all could analyse a given chord under attention; but very few could name its constituent parts. All appreciated good music in a fairly high degree; the writer believes that there is sufficient evidence for the statement that in all cases the aesthetic judgment was well developed in the field of music, and that there was real musical enjoyment. Most of the observers hum, whistle, or sing melodies to themselves, ordinarily, although they may at times imagine melodies in terms of auditory-kinaesthetic complexes. For some of them, organic and visceral sensations play a large part in the appreciation and recall of musical experiences. In these cases the rhythm may be conveyed "in indefinite kinaesthesis." Another found "muscular sensations in body, neck, and head which come from an involuntary attempt to keep time and to use bodily movement to help me do so." Under the instruction to recall a concert or recital, and to report upon the consciousness thus aroused, almost all of the observers got

associative visual, kinaesthetic, auditory, and auditory-kinaesthetic imagery. "Then auditory-verbal images, 'Evening Star,' and auditory imagery of certain parts of the selection, *especially* of the loud clarinet tones, strain sensations in throat, and kinaesthetic imagery in arms (playing piano) with vague auditory images of piano notes accompanying clarinet. Visual images of clarinet player and his chair, localised in part of the room." "Auditory images and kinaesthetic images in throat (of the notes) and kinaesthetic sensations in right foot and ankle (the latter meaning 'keeping time,' and also 'an aid to recall')." "Also vague auditory imagery of high-pitched notes for the 'swings.' Then verbal idea: 'and symphony—Beethoven.' Vague visual imagery of Boston Symphony Hall." "The present memory of this song is a visual image of Bispham on the stage, open mouth very clear, with two very clear successive throat-feels, meaning a high and a low note respectively. An auditory component accompanies the upper note but the lower one is entirely kinaesthetic." "My memory consists of vague visual images of the players and strong kinaesthetic images of bodily position."

The value of these answers as introspective material for our experiment is, of course, very doubtful. The material was, for one thing, not obtained under experimental conditions of control; for another, it has all the shortcomings of a questionary method of attack. It did, however, orientate the writer in regard to his observers, and gave him an estimation of their value in a series of experiments of this nature.

At one time in the series, when the investigation concerned itself with rhythms whose constituent members differed physically in pitch, it became advisable to test the observers who took part in that particular problem in regard to their ability to discriminate pitch. The *DL* was accordingly determined for D, E, and G, by the *method of limits*. Two Spindler and Hoyer forks pitched at a^1 (435 vd.), with attached automatic hammers, were used,[43] and 40 series, in addition to the usual number of preliminary series, were taken. The results gave: *DL* for D = .4 vd., for E = 1.82 vd., and for G = 1.86 vd. These results average below the mode obtained by Mount and Smith in investigations with 781 undergraduate men and women.[44] Their results taken from crude tests with steps of one or more vd. show the greatest percentage of frequency at 2 vd. $(21 + \%)$ for a' of 435 vd. Our steps were uniformly equal to .65 vd. In comparison with what has been found in psychophysical experiments of this sort, our results are a trifle high, indicating relatively poor pitch discrimination (with the possible exception of D). Luft, by the same method used on himself, found a *j.n.d.* of .251 vd. at c'' and one of .232 vd. at c', the octave which includes our own determinations.[45] Titchener says: "The value of the *DL* (absolute) as obtained under the described conditions has never exceeded 2 vs. for either set of forks, and has fallen as low as .75 v."[46] While, therefore, in terms of Luft's results our observers were on the whole poorer in pitch discrimina-

[43] These are listed in catalog no. 21 as no. 130b, but riders and automatic hammers are not listed as attached.

[44] *V.* Seashore, C. E. The measurement of pitch discrimination, *Psychol. Rev., Monog. Suppl.*, 13, 1910, 43.

[45] Luft, E. *Philos. Stud.*, 4, 1888, 511.

[46] Titchener, E. B. Experimental psychology, New York, 1905, vol. II, pt. II, 126.

tion, nevertheless, in terms of Titchener's statement, one of our observers, D, was below the .75 v. mentioned, *i.e.*, better, and the other two were not unfavorably comparable with the worst.

C. Apparatus.—It is almost inevitable, at the present stage of investigation, that the arrangement of apparatus for the adequate control of experiments in acoustics, and especially in the field of auditory rhythm, should be complicated. Parts of the apparatus must be constantly modified to suit new conditions, other parts must be invented for the same purpose. In the early preliminary experiments an ordinary Maëlzel metronome was used. It was, however, carefully selected with a view to eliminate any qualitative differences in the beats. The settings used gave: 42, 48, 66, 92, 152, 176, and 200 beats to the minute.

The metronome was placed on a heavy piece of harness-felt and covered over on all sides, except on the side toward the observer, with an inner lining of cotton batting and an outer layer of harness-felt, held in place by a caging built up of wire supports. This cage measured in all about 70 cm. in diameter and 40 cm. high. It prevented the possibility of echoes from the walls of the room, while it at the same time permitted the sound of the metronome to be heard clearly by the observer, who sat with his back toward the opening of the cage, 2 m. away. This arrangement, of course, also eliminated to a great extent the clang elements present in the ordinary metallic click. Rubber pads on the chairs in the room and felt pads on the feet of the table used for the metronome made occasional movements of these pieces of furniture practically noiseless. A contrivance for starting and stopping the metronome when the experimenter was at a distance away from the instrument was used. This consisted essentially of a wire bar padded with felt and fastened on one end directly to the side of the cage at its front opening and, on the other end, by means of a rubber band to the opposite side of the cage. At the latter end a piece of string was attached which the experimenter held in his hand. The pendulum of the metronome was then placed at one of its extreme positions against this horizontal bar. When the string was pulled, the pendulum was released; when the string was released, against the pull of the elastic rubber band, the bar sprang back in place and caught the pendulum. With a little practice the experimenter was able to catch the pendulum at one of its extreme positions, so that the next pull of the string would find the pendulum ready to begin an excursion. A contrivance of this sort was found necessary because of the advisability of having the experimenter near the observer, while the latter was observing the rhythm produced, in order to detect, if possible, any movement of exposed parts of the body, or any change in breathing. The crudity of such a method of experimental control of the observer is realised and acknowledged, but the series was of so preliminary a nature that it was not considered necessary to take detailed and accurate means of registration until the problem had been clearly outlined.

In the second part of the preliminary series, where an objective rhythm was produced in terms of differences of in-

tensity of sounds, a Titchener rhythm-box was used in connection with the metronome.[47] But, like the latter instrument, this rhythm-box had also to be modified to meet our requirements.

It became necessary to begin a rhythmical series on an unaccented beat, *i.e.*, with an unintensified member of the group; it was also imperative that a rhythm be started and stopped at will. For this reason a releasing and arresting device was attached to the box. The principle of this was practically the same as that used in connection with the metronome in the first part of the preliminary series. Its operation was entirely noiseless. A full account and an illustration of the attachment has already been published.[48]

The observer sat in the same position in this second series of the preliminary set as in the first, and in all other respects the physical conditions were the same.

In the main series of experiments a complete change of apparatus and of the general experimental conditions was necessary. It was our intention to work as far as possible with pure tones combined to produce a two-membered group. These tones were to be variable within certain limits in pitch, intensity, and duration. Two adjacent rooms in the center of the upper floor of the laboratory, and therefore comparatively free from disturbing noises, were used for the rest of the experimental series. One of these rooms, an inside dark-room, in which the observer sat, was separated from the other, in which the experimenter and the greater part of the apparatus were placed, by a heavy stone wall 50 cm. thick. Since the variability of the tones produced, in regard to pitch, intensity, and duration, was the factor that separated the entire main series of experiments into smaller divisions, it will be well to designate these divisions in some arbitrary manner. If we call the two large divisions of the preliminary series, the one concerned with subject rhythmisation, the other with objective rhythmisation, respectively *A1* and *A2*, then we may call the divisions in the main series, *viz.*, one concerned with objective differences of duration, another with differences of intensity, and the third with differences of pitch, respectively *B1*, *B2*, and *B3*.

In the experiments *B1*, two tones were produced, whose relative duration was variable. In all cases the intervals between the tones were constant: one short interval, and one long interval of approximately twice its length, followed one another in alternation, *e. g.*, tone (variable duration)—short interval—

[47] Listed in Stoelting's catalog, Dec., 1909, as no. 7414.

[48] Bentley, M., Boring, E. G., and Ruckmich, C. A. New apparatus for acoustical experiments, this *Journal*, 23, 1912, 513.

tone (variable duration)—long interval—cycle repeated, *etc.* The intervals were kept uniformly constant in duration throughout all of the series *B1*, *B2*, and *B3*. It must also be understood that whenever mention is made of the variability of one of the factors of the tones, *i. e.*, pitch, intensity, or duration, the other factors, not spoken of as variable, are constant. A tone, for instance, which is variable in duration, is not variable in pitch or intensity.

For meeting the conditions of the *B1*-set of experiments, a fork of Koenig manufacture and belonging to the Helmholtz set[49] for synthetically reproducing vowel sounds, was enclosed in a sound-proof box. The fork of 256 vd. was used with its cylindrical resonator because it produced the clearest and best-carrying tone of the set. Connected electrically in series with it, but, of course, outside of the sound-proof box, the exciting-fork, from the same set, was utilised for the purpose of keeping the *c'*-fork in vibration. Opposite the opening of the resonator of the *c'*-fork, a telephone transmitter of especial construction was placed.

The transmitters and receiver used in these experiments were made with a view to the most faithful, but not the most intense, reproduction of sound and had been chosen at the recommendation of a member of the Department of Physics. The type of transmitter was no. 227W and that of the receiver was no. 128W, both made by the Western Electric Co., of New York City. That the sound produced by this telephonic circuit was normally tonal in character is shown by the following quotations selected from the reports of the observers when the apparatus was working under standard conditions: (D) "Noticed tonal difference and began saying 're, ti,' 're, ti,' *etc.*" At another place D refers to the sounds as being 'musical' and 'bell-like.' (E) "Auditory sensations from first stimulus, perceived as tone; but I am unable to perceive the rhythm." "Tones seemed to hold the center of consciousness this time." "Tones were immediately reproduced auditorily—it seemed as though I could hear my own voice singing them." G remarks: "Now feeling of doubt became clearer especially in the period between the groups of tones—at other times general trunk kinaesthesis would be clearer—always clearer when tone was pleasant." "At these times, auditory sensations of tone, a kinaesthetic swing of the trunk with each auditory sensation." B: "Auditory perception of first tone, accompanied by kinaesthetic strains in chest." "Then auditory perception of second tone interrupted it. I do not know what happened to kinaesthesis then (as I think I was surprised at quickness with which second tone appeared)." F: "Attention went from the tones to visual image of paper on which I was to write." "Now attention goes back to louder, lower, tone (*i.e.*, after judging) and it immediately became first tone of a foot, I had lost one tone." In addition to these statements, it may be said that the observers generally had no

[49] For a description of this apparatus *v.* Helmholtz, H. The sensations of tone, (trans. Ellis), 2nd ed., London, 1885, 119-23.

trouble in assigning a pitch to the sounds produced. That the tones produced were wholly free from clang-quality can not be asserted. When the entire apparatus was working at its best, however, a tone with only a few clang elements resulted.

The telephonic circuit which embraced the transmitter passed out of the sound-proof box through a suitable induction-coil and storage battery. From the secondary of the induction-coil, it was led to an interrupting apparatus of special design.

Various types of interrupting devices were tried out only to be discarded. The Meumann time-sense disc, geared to the Ludwig-Baltzar kymograph, was tried in connection with small interrupting magnets for making and breaking circuits relayed from the time-sense disc, but difficulties arose in regard to the improper response of the magnets owing to hysteresis. Later an Edison phonograph with an electric motor whose speed was kept constant by means of a ball-governor was substituted.[50] The governor automatically shunted the current through a resistance-coil when the speed tended to increase. On the cylinder of the phonograph a wax-record was mounted. The record was covered in the following manner: over a little less than one-half of its circumference and extending the whole length, a strip of smooth white paper was pasted; the remainder of the surface was covered with two strips of triangular shaped tin-foil, separated by an oblique section of white paper whose width was equal to one-half of the wider strip of white paper. The triangles were equal right-angle triangles and their hypotenuses were adjacent to the oblique strip of white paper; they were inverted and apposed so that the point formed by the union of the longer arm and hypotenuse of one was in circumferential line with the shorter arm of the other, and *vice versa*. In place of the ordinary style and diaphragm, a contact-arm ending in two platinum-wire fingers was substituted. These wires were connected through switches to the secondary circuit of the telephonic system, so that whenever the two fingers passed over a section of the tin-foil the circuit was completed. As the record was rotated, then, these fingers would pass, for instance, at the extreme left of the record, over the circumferential width of a strip of tin-foil nearest the tip of the first triangle, making a very short contact, then over the width of the oblique section of white paper, making no contact and, therefore, the interval between the two members of the group, then over the width of the second strip of tin-foil, this time nearest the shorter arm, and, therefore, comparatively a long contact, and finally over the widest strip of paper, giving the long interval between the groups: in all producing an iambic rhythm. By shifting the arm along the length of the record, gradual changes, from iambic at the left, through spondee in the center, to trochaic at the right, could be effected. The amount of these changes was read off from a millimeter scale on the record. It is understood, of course, that the intervals, both within and without the group, remained constant. The times of these component factors of the rhythm-group were: first member, variable from 0″ to .6″, the interval between members constant at .5″, the second member, variable from .6″ to 0″, the long interval

[50] A sketch of this type of phonograph can be found in the Standard Dictionary, edition of 1908, under 'phonograph,' pg. 1329.

between groups, constant at .9″; in all, the time for a complete cycle was 2.0″.

This method of interrupting the circuit had to give way to another, because the contacts between the platinum points and the tin-foil were not of the best. Almost invariably a scraping noise could be heard in the receiver. To eliminate this, a rotating cam-device for interrupting the circuit was resorted to. The phonograph-motor was still used, but its cylinder-drum was used as a driving pulley with a belt-gripping attachment. The belt passed over the pulley of the rotating cam-device and allowed triggers to drop alternately into small mercury cups. A complete description of the mechanism and use of this apparatus has already been published.[51] It has since been slightly modified with respect to the operation of the triggers. They are now made to drop more quickly and more deeply into the mercury cups, which are placed side by side, in that their power-arms are comparatively longer than before; since they are now made of heavy piano-wire, and the contact points are therefore sharper and thinner, better contact is made. With this device the time values as shown on kymographic records, a 50 vd. fork writing the time-line, were as follows: first member, variable from 0″ to .44″, the interval between members approximately constant at .45″, the second member, variable from .5″ to 0″, and the interval between groups approximately constant at 1.0″; in all, the time for a complete cycle was approximately 1.8″. Kymographic records were also taken in the same way for the purpose of controlling the speed of the electric motor at various points on the scale of the sliding rheostat. The speed of the motor was regulated to keep the time of the cycle of the rhythm between 1.8″ and 2.0″ throughout all of the series.

The secondary circuit of the telephonic connection passed from this interrupting device, through suitable switches and through a tube in the wall, to the dark-room where it ended in a receiver of the type mentioned. A head-band was attached to the receiver. The observer wore this receiver continuously throughout a single experiment over the better ear, if there was any choice. It fitted very comfortably and its presence was usually hardly noticed.

The system of signals was arranged so that a light-flash from a 4 c.p. incandescent electric light prepared the observer for the rhythm.

It was agreed that the light should remain on during the period immediately preceding the experiment. When the experimenter was ready to begin, this light was turned off. When the observer was ready, he pressed a button which rang a muffled bell in the experimenter's room. After three seconds from this signal, the switches which started the rhythm were thrown. A double-arm, double-throw switch connected at a single movement both primary and secondary circuits. This was done to insure against leakage from one circuit to the other. It was necessary to keep the wires which carried the alternating current for the signal-light well out of the way of the telephonic circuit, because it was found that the receiver circuit would detect with the greatest delicacy any electric variations within

[51] Bentley, M., Boring, E. G., and Ruckmich, C. A. *op. cit.*, 511-13.

a meter if the systems ran parallel for more than about 30 cm. The observer's eyes were protected from the light by a screen.

The experimenter's room was connected with the observer's room by a speaking tube through which special instructions were given and introspections reported. A stop-cock disconnected this tube while the rhythm was being produced, in fact, through the whole period of observation.

For the *B2*-set of experiments, where the members of the rhythm-group were changed in regard to intensity, only slight alterations in the apparatus were necessary. A second fork, manufactured by Max Kohl, of the same vibration-rate and of the same tonal quality as the first was enclosed in another sound-proof box. Opposite to its resonance-box a transmitter, of the same type as the first, was placed. The telephonic system mentioned above was duplicated so far as its primary circuit was concerned. In one of the primary circuits, however, a 111 ohm resistance-box was inserted.[52] By pulling various plugs, resistances from .1 ohm upwards could be introduced. In this series of experiments, the primary circuits were led to the interrupting device.

New cams had to be made to suit the new conditions. Instead of two shifting cams operating on the same trigger, two cams were made with the same contour, *i.e.*, they were in every respect, save in position on the spindle, identical. In mounting them on the spindle, they were separated about one centimeter and arranged so that their depressions gave a spondee rhythm of the same kind as the other cams had produced in terms of duration when they were operating together, *i.e.*, each cam produced a single rhythmical member whose duration was equal to the duration of one of the members of the rhythms produced in series *B1* when the cams were set at the spondee adjustment; but since they were tripping levers which dipped into separate cups of mercury corresponding to the two circuits of the primary telephonic circuit, both cams together produced a rhythmical group which was spondee in terms of duration, as in series *B1*, but was variable in terms of intensity, depending upon the adjustment of the strength of the current relatively in these two circuits. When the resistance or, what amounts to the same thing, the current was equal in both circuits, the result was a spondee rhythm,[53] when the intensity of the current corresponding to the first connection made was greater than that of the other, a trochaic rhythm would be produced; and when the intensity of the current corresponding to the last contact made was greater than that of the first, an iambic rhythm resulted. In these experiments, the secondary circuit passed directly from the induction coils of the telephonic system to the receiver in the adjoining room without, of course, entering the interrupting mechanism.

[52] This type is listed in the Physical and Chemical Catalog No. 23, March, 1912, issued by the C. H. Stoelting Co., on page 140 as No. 2939.

[53] The point of subjective equality was determined by a long series of judgments given by all of the observers concerned.

It is evident, then, that in this series, a change of intensity in the two members could be effected by throwing resistances into the primary circuits, without thereby changing the constancy of the durational component.

In the *B3*-set of experiments, no change was made in the interrupting device. Since the series occasioned variations in pitch between the two members of the group, while the durational and intensive factors remained constant, the forks hitherto used had to be replaced by two forks of identical construction, mounted with riders, and giving a range of about 76 vd., from 228 vd. to 304 vd., a major third (a♯-d′♯).[54] They were electrically self-exciting, but the noise of this excitation was not audible through the telephonic receiver, mainly, perhaps, because the transmitter was mounted opposite the resonator of the fork and therefore not in the immediate vicinity of the slight noise produced by the excitation. The forks were about as free from clang elements as the others had been in the other series. The resistance-box, which had done duty in the variation of intensity in the previous series, was cut out of the circuit, and the cam-device for interrupting the primary circuit of the telephonic system was left unchanged.

The forks were equated for intensity as nearly as possible by making the physical conditions identical. This was practicable in this set of experiments because all the elements were the same. In the previous series, different forks were used, the transmitter had therefore to be in a relatively different position for each fork, and the wiring had to be different. An extended set of judgments on the relative pitch, intensity, and duration of the two forks was, nevertheless, undertaken for the sake of practice on the part of the *O*'s and with a view to correct any possible wrong adjustments. We also had difficulty here, as well as in previous sets, with complications produced by one attribute on the adequate judgment of another. Judgments of intensity would very often be confused with judgments of pitch and *vice versa:* but more of this in another section.[55]

Pneumographic records were also taken in this set of experiments. The Verdin pneumograph was adjusted to *O* for chest expansion, rubber tubing was then led through the wall to a Verdin tambour which, together with a Jaquet clock, recorded on the drum of a Ludwig-Baltzar kymograph during the period of the experiment. Later a Kronecker interrupter was used at the 1/25 sec. setting in place of the Jaquet clock

[54] These forks were of a new type, made to Titchener's specifications by the C. H. Stoelting Co., of Chicago.

[55] Stumpf enters into a discussion of this difficulty in his *Tonpsychologie,* Leipzig, 1883, I, 347.

at 1/5 sec. because it was considered desirable to get a more accurate reading.[56]

At first the kymograph was placed in the room with *O*, but we soon found that there was considerable distraction caused by the noise of the kymograph-motor. For this and other reasons it was taken out of the room.

In a short series of experiments, light-flashes were used to produce a rhythm. Let us call this series, *L*. Connections were made from a source of direct electric current at about 100 volt pressure through one of the levers of the interrupting device, and from the same source through a resistance, bringing the pressure down to about 50 volts, then through the other lever of the interrupting device. From this device the wires passed on through suitable switches to the adjoining room where they ended in a Mazda incandescent light of 40 watt consumption. This light was placed in a small box, 17 x 17 x 20 cm., whose inner surface was lined with white Bristol board in order to reflect as much light as possible. On one side of the box was a circular opening 1 cm. in diameter, which was covered with translucent architect's tracing paper. This box was placed midway between *O* and a white screen a trifle over a meter from *O* so that the light from the opening in the box was reflected from the screen, covering an area about 60 cm. in diameter.[57]

The opening of the box was, of course, toward the screen. The screen was bounded by black cloth, which limited the field of illumination to some extent. A 4 c. p. incandescent light was allowed to diffuse its light from a point two meters behind *O* over the screen in order to produce conditions unfavorable to the formation of after-images. *O* was allowed to regard the screen in the period before the experiment began and in the period of preparation. The rhythm produced was, as were all the rest, two-membered, in which either the first or the second flash was the more intense—and also, owing to the conditions, the more extensive—of the two, or with another arrangement of the wiring, both were of equal intensity (and extensity), *i.e.*, the interrupting device was so arranged that it would deliver a current through the Mazda light of 100 v., then 50 v., *etc.*, or 50 v., then 100 v., *etc.*, or 100 v., then 100 v., *etc.*, or 50 v., then 50 v., *etc.* The Mazda filament lends itself very well to this sort of flash, because it reaches its maximum illumination or glow very quickly (in about .05 sec.). The receiver was naturally disconnected from the telephonic circuit in this particular series.[58]

[56] This interrupter is of the lamella type and is made by G. Hasler, Bern.

[57] The conditions were approximately those of the experiments in light-rhythm by Koffka, *op. cit.*, 6.

[58] After reading this account of apparatus used in the experiment, the importance of physical instruments in our investigation will doubtless be realised. That at least one recent writer challenges our

D. Method.—In these experiments over 700 observations were taken during a period of two years, from February, 1911, to February, 1913. Each one of these observations was a detailed introspective account of consciousness during the period of the presentation of the rhythm. Three hundred and sixty-five of these were in the preliminary series, which covered about one half-year's experimentation; the remainder, 313, much more detailed and analytical in character, extended over the remaining year and a half.

In the preliminary experiments, series *A1* and *A2*, subjectively accented and objectively intensified rhythms were investigated. In the series *A1*, which dealt with subjective rhythms, the first instruction used was:

(*a*) To assume mentally and physically as relaxed a condition as possible: to give a general introspective account of the rhythmical consciousness, with special reference to the process of grouping of the metronome-beats and any change that may occur in that process.

Under this instruction B dictated 17 introspections, Bu 21, D 20, and Ge 4. With every *O* except D the metronome was allowed to run for 45 sec. and the introspections were taken after that period. After the first trial, D having remarked that the changes in her rhythmical consciousness were so numerous that she could not well remember them, her introspections were taken after a 15 sec. play of the metronome. All of the *O*'s preferred and were allowed to keep their eyes closed during these experiments. Noticing that most of the introspections centered about factors relating to accent, we considered it advisable to focus introspection more closely upon this point. To this end the instruction was changed to the form:

(*b*) To notice in terms of what psychical factors accent is determined.

Under this instruction B dictated 1 introspection, Bu 15 introspections, D 12, and Ge 8.

In the second set of experiments, *A2*, objectively intensified sounds were presented to *O* in rhythmical groups by means of the metronome and rhythm-box.

Again we followed in the main, as we had done in the A^1-set, the suggestions in Titchener's *Manual*,[59] but evidently for a different

right to grant this importance, we may gather from a passage in Verrier's *Old Testament and Semitic Studies*, Chicago, 1908, 177: "It is almost superfluous to point out in this connection, that facts which require instruments for their discernment have no place in the study of rhythm."

[59] Titchener, E. B. Experimental psychology, Vol. I, pt. I, 176-7; pt. II, 339-47.

purpose than that outlined in the text. Our aim was not so much to discover what form the group assumed or what value, in terms of intensity, duration, or quality, was assigned to the various members of the group, but to obtain a fairly accurate description of the constitution and pattern of the rhythmical consciousness under the influence of the perception, with different degrees of difficulty, of the objective rhythms described below. Bolton had come to the conclusion that the normal group is the two-group; more difficult is the three-group, or the twice-three-group; and still more difficult is the five-group.[80] The rhythms that we used were: (′— —), (— ′—), (′— — —), (— — ′—), (— ′— —), (′— — — —), (′— ′— — —), (— — — ′—), (′— — — — —), (′— — ′— — —), (′— ′— — ′— —). Each group occurred twice at three different rates each time (152, 176, 200 per min.). The consecutive order was haphazard, though definitely outlined before the experiment was begun. This means that an easy or difficult rhythm was presented to *O* without his foreknowledge as to the degree of its difficulty. The rhythm usually lasted 15 sec. At the very end of the series, this time was changed for reasons given later.

This *A2*-set of investigations was considered the more important of the two preliminary sets because, in the first place, the *O*'s had gained practice in the introspection of the rhythmical consciousness in the course of the first set; and again, because the experimenter took greater pains to concentrate the instruction on such crucial points of attack as had already been suggested in the course of the *A1*-set and in the early part of the *A2*-set; and finally, because more introspections were taken and a longer period of time was spent on this set.

In these as well as in previous experiments, the usual 'ready' signal was verbally given. The introspections were then taken down word for word and are preserved *in toto* in the protocol. The first instruction was:

(*a*) To describe the rhythmical consciousness, giving especial attention to (1) the ease with which the particular rhythm is perceived, (2) any change in the rhythm that may occur, (3) the placing of major and minor accents, and (4) the duration of the beats.

Under this instruction Bu dictated 35 introspections, D 24, and Ge 20. Toward the end of the series, it was suggested, especially in the introspections of D and Ge, that there was some change in the nature of consciousness during the period of the playing of the rhythm. This induced us to turn the instruction into the form:

(*b*) To observe any changes that may occur in the course of the rhythmical consciousness.

Under this instruction Bu dictated 15 introspections, D 16, and Ge 20.

Believing that we could get much better results by resorting to the method of 'fractionation,' well-known in the reaction

[80] Bolton, T. L. *op. cit.*, 212.

experiment, we divided the whole period into two parts, called respectively the 'fore-period' and the 'after-period.'[61] The former began with the objective playing of the rhythm and ended with the clear perception of the rhythm on the part of *O*, at which time the objective rhythm was stopped at a signal from *O*. The latter began with a clear perception of the rhythm and ended with the close of the usual 15 sec. period.

It must be recognised that (1) the dividing point in this fractionation, if arbitrary, was well suited to our investigation because we were hoping to find the critical phenomena which centered about this point, and (2) that it was necessarily indefinite, because the recognition of the rhythm may be gradual and not climacteric, but that with practice this fact presented no unusual difficulty. While a sharp, distinctive fractionation of the period was ideally aimed at, a slight prolongation of the fore-period or the after-period did no serious harm.

Bu gave 9 consecutive introspections under the instruction covering the fore-period, D 15, and Ge 13. The instruction read:

(*c*) To observe the rhythmical consciousness until the moment when the rhythm is clearly perceived (at which time a signal is to be given to the experimenter to stop the objective rhythm), giving no attention to the report of the kind of rhythm perceived, but making sure that the kind is definitely determined.

Then Bu, D, and Ge each gave ten introspections under the instruction pertaining to the after-period. The objective rhythms previously reported on were repeated for these introspections. The instruction was:

(*d*) To observe the rhythmical consciousness after the moment when the rhythm is clearly perceived, giving no attention to the report of the kind of rhythm perceived, but making sure that the kind is definitely determined.

Convinced that this method had given enough practice in the recognition of the dividing-point of the entire period, we repeated, in the course of a few weeks following, the rhythms used before, but with instructions *A 2 c* and *A 2 d* arranged in haphazard order.

A direct question was then put to each *O* in the form:

(*e*) What difference, if any, do you think, is there between the

[61] For a discussion of the advantages and disadvantages of this method *v.* N. Ach, Ueber d. Willenstätigkeit u. d. Denken, Göttingen, 1905, 19 f., and G. E. Müller, Zur Analyse d. Gedächtnistätigkeit u. d. Vorstellungsverlaufes, Leipzig, 1911, 75 ff.

rhythmical consciousness under instruction *A 2 c* and the rhythmical consciousness under instruction *A 2 d*?[62]

Finally the rhythmical consciousness was allowed to degenerate, as it were, under instruction *A 2 d*, by the lengthening of the period from 15 sec. gradually through stages of 20, 25, 30, 35, 40 and 45 seconds duration to the extreme length of 1 min. The *O*'s were not told the purpose of these gradual increases in duration, and were generally and quite surprisingly unaware that the period had been lengthened. D gave 11 introspections, and Ge 7. These experiments ended the preliminary series.

With the commencement of the main series of experiments, there was a complete change of apparatus, and, owing to the beginning of a new academic year, there was also a change of *O*'s. D was the only *O* who continued to serve after the preliminary series. In this part of the main series, which we shall call *B 1*, a two-membered rhythm was produced, the difference in whose members was one of duration—other physical factors were constant. Our aim in thus separating these factors was to isolate our conditions as far as possible and to see whether, when we had made these conditions as simple as we could, the facts discovered would be the same or different for each analysis. A 15-sec. period was used, which gave time for about 8 complete rhythmical cycles. Ten different changes in the relative duration of the two members were within the limits of the efficiency of the apparatus, and these were made in haphazard order. The first instruction was:

(*a*) To give an introspective analysis of the perceptual consciousness.

D gave 15 introspections, E 8, and G 12. Noticing that, in spite of the fact that there were no differences in pitch and intensity between the members as they were physically produced, the *O*'s were frequently reporting such differences, we saw no way of avoiding the giving of full and explicit instructions as to the physical possibilities involved. The next instruction, therefore, was:

(*b*) The sounds which you will hear will be objectively of the same intensity and pitch but will vary in duration as follows.

[62] This is a sort of modified 'method of confrontation' as it was used by T. Okabe, in his *An experimental study of belief*, this *Journal*, 21, 1910, 590. For a criticism of the method *v.* K. Koffka, *Zur Analyse d. Vorstellungen u. ihrer Gesetze*, 1912, 21.

Then in every instance the type of rhythm was announced. This instruction was supplementary to *B1 a*. D dictated 7 introspections, E 8, and G 2. But the *O*'s persisted in making the confusion of judgments of intensity, pitch, and duration. A series of judgments was then given with sole regard to the estimation of pitch, intensity, and duration of the second member in terms of the first. Using the settings which corresponded to judgments of equal intensity and equal pitch in each individual case, and with the knowledge on the part of the *O* that this was being done, we repeated instruction *B1 a*. This resulted in 5 introspections from D, 5 from E, and 10 from G.

We found, after we had taken this number of introspections, that two of the *O*'s had come to think that the experimental conditions were not what the experimenter said they were, and that he, perhaps, was trying the effect of suggestion on the perception of rhythm. To meet these conditions the following instruction was given:

(*c*) Throughout the series of rhythmical experiments, suggestion is not resorted to; the knowledge given is in terms of true objective conditions.

Under this instruction D gave 8 introspections, and E 2. After this, we allowed the rhythm presentation to continue for 45 sec. and asked for as detailed an account as was possible under these circumstances. D gave 20 introspective reports, E 6, and G 24.

In order to get at the differences in conscious pattern during the course of the period, we again resorted to the method of 'fractionation,' dividing the total period into three parts as the following instructions will show:

(*d*) Give a minute analysis of consciousness during the period extending from the 'ready' signal to the first auditory impression; and report upon the pitch, intensity, and duration of the members of the group presented.

(*e*) Give a minute analysis of consciousness during the period extending from the first auditory impression to the time when the two auditory impressions are perceived as belonging to a group; and report upon the pitch, intensity, and duration of the members of the group presented.

(*f*) Give a minute analysis of consciousness during the period extending from the time when the two auditory impressions are perceived as belonging to a group to the end of the experiment; and report upon the pitch, intensity, and duration of the members of the group presented.

D gave 10, 7, and 9 introspections under the above instructions, respectively; E gave 4, 4, and 1; and G gave 5, 6,

and 8. A few introspections among these were taken under the last instruction while the period was lengthened gradually to 1 min. Finally, the above instructions were given in haphazard order, to make sure that the *O* was not getting practised to the point of giving a 'habitual report,' *i. e.*, in stereotyped form. D gave 24 of these introspections, and E 9.

In the *B2*-set of experiments, changes were made in the relative intensity of the two members of the rhythm-group, while the other factors remained physically constant. The most frequent settings of the resistance-box which introduced these changes of intensity were 0 ohms, 2.5 ohms, 3 ohms, 4 ohms, 10 ohms, and 20 ohms. At first all the *O*'s were called upon to give a series of judgments on the relative duration, pitch, and intensity of the two members. Then a general instruction was given:

(*a*) Give a detailed introspective account of the perceptual consciousness and report upon the relative intensity of the two members.

D gave 2 introspections, E 4, and G 14. D was then asked to 'fractionate' her reports as in *B1 d*, *e*, and *f*. She gave 7 introspections under this instruction. G gave 3 on the first period only. D was also asked:

(*g*) To introspect consciousness during perception with as passive an attitude as possible.

She gave 8 introspections as the result of the instruction. A series of additional judgments was then taken on account of a slight improvement in the apparatus. Finally D and E were asked:

(*h*) To give an introspective account of consciousness; and to report upon the relative pitch, duration, and intensity of the two members.

Under this instruction, the period was made as long as 1 min. D and E gave 3 introspections apiece.

In the third set of experiments (*B3*), changes in pitch were effected, while the other components remained constant. The changes were again made in haphazard order and ranged from a♯-d′♯. At first a long series of judgments was taken in order to secure practice in the discrimination of the relative pitch of the two sounds. Then the method of 'fractionation' was at once resorted to, and since the instructions were similar to those of *B2 d*, *e*, and *f*, we shall designate them in the same way, *B3 d*, *e*, and *f*. B gave 7, F 5, and G 8 introspections in all. Since this series of experiments

began at the commencement of a new academic year, a change of *O*'s was made necessary. This accounts for the presence of B and F as *O*'s in this part of the series. Finally instructions *B3 d, e,* and *f* were given in haphazard order. B gave 8 introspections, F 13, and G 10.

In order to get at the significance of kinaesthetic factors in the perception of rhythm in a slightly different manner, we aimed to find out what would happen to the perception of rhythm when an instruction was given to suppress these factors. Accordingly the following was given:

(*x*) Report upon the course of consciousness while inhibiting kinaesthetic processes which are relevant to the rhythmical grouping of the auditory perceptions.

This was done with a pitch-rhythm as a stimulus. B gave 2 reports, F 5, and G 4.

To discover whether what we had found in regard to auditory rhythms would also hold true of visual rhythms, we tried a short series with light-flashes differing in degree of intensity, and forming a two-membered group on the same pattern as the auditory rhythms. After a series of preliminary judgments with respect to the relative intensity of the two members, the instruction was given:

(*a*) You will see a series of flashes on the background opposite you, which background you will fixate upon. Report your total consciousness, and judge the relative intensity of the two flashes.

Under this first instruction of this series, which we will call *L*, B wrote 2 introspections, F 5, and G 6. The instruction was then 'fractionated' as before. Under these instructions, which were at first given in order and then in haphazard arrangement, and which we will label *L d, e,* and *f*, B wrote 5 introspections, F 7, and G 6. It will be remembered that in the description of apparatus we mentioned that pneumographic records were taken in both series, *B 3* and *L*. In one or two places, supplementary questions were asked where the introspection was not perfectly clear. These questions will be given in connection with the introspective results.

At the end of the entire investigation, another 'confrontation' question was put:

(*z*) Can you describe, in a general way, the course of kinaesthetic processes during the period of the experiment?

To this question B, D, F, and G gave detailed answers.

III. Results

A. Preliminary Series.—Under the first general instruction, *A1 a*, we obtained the following typical introspections:

(B) A two-group with decided pauses between the two members. There is a difference between the two members which does not seem to be qualitative, intensive, or durational. I wondered what this was. I discovered very vague kinaesthetic or organic sensations in chest and abdomen. These are different for first member and second member, but it is not clear how. First and second seemed to sound alike but *meant* something different due to these organic sensations. Toward the end, idea of four-group suggested itself. This was followed by hearing of four-group of two pairs. I do not think that the pause between the four-groups was longer than the pause between the pairs of one four-group. At the end of the four-group, however, there were kinaesthetic sensations in trunk meaning, 'this is the end of it.' These sensations were more intense than those accompanying the hearing of the preceding beats.

[*Rate: 48 per min.*]

(B) First beat, then long pause, then second beat. Anticipated second beat with the idea of the two-group. Two-group followed and continued when second beat came. Long pause between groups. Very little accent, if any, on first beat. Kinaesthetic sensations in head and body, repeated apparently the same in each group. Vague feelings of swaying (leaning forward) to left in time to the two members of the group. When the idea occurred, it came as a visual image of the leaning. Qualitative differences of the beats discovered toward middle of the experiment. I had idea (not verbal), 'will this change the grouping?' Four-group suggested: next pair became attached to the preceding pair, making the four-group. Four-group continued until the end. The pairs of the four-group seemed exactly like the individual two-groups before,—only that the thing that made the four-group was that one pair persisted as image during the sounding of the second pair. The image was very clear,—probably a memory-after-image. This was in terms of auditory images. There were also less clear visual images consisting of two brown dots on a gray background—meaning the two beats. As soon as the group is completed, the whole complex of the four-group vanishes at once. There seems to be no image persisting from the second pair of the four-group. [*Rate: 42 per min.*]

(Bu) Four-group with accent on third beat. Class in calisthenics suggested in terms of visual imagery. Time was considered good for this but still a little slow. Watched the exercises go on with kinaesthetic reproduction in arms. Auditory imagery of music. Strong visualisation of girl at piano. I think my foot moved three or four times. [*Rate: 42 per min.*]

(Bu) Four-group with accent on three. It kept singing itself (auditory imagery) like a hymn. Later said: 'well done' like a chant. Foot movements corresponding to the four beats. First and third beats were pitched. Finger moved also. I tried to get rid of 'well done' suggestion. [*Rate: 42 per min.*]

(D) Two-rhythm with accent on first beat. Kinaesthesis in throat without articulation and occasionally with counting, 'one, two.' Head moved according to rhythm—down for accent, brought back for non-accent. Strain for accented 'one,' relaxation for unaccented

'two.' Breathing in on 'one' and out on 'two.' There were qualitative changes in the experiment. Visualised soft brown spot on one accented beat. For most of the time, visualised a solid figure of steel-gray-bluish color with angles. More of the accented ones were seen and they were larger. For the first two or three beats there was a tendency to localise the sound behind with a corresponding kinaesthetic strain in the neck. First two or three beats were not rhythmically grouped. Comforting and pleasant rhythm. Feeling of relaxation. [*Rate: 92 per min.*]

(D) 'Tick, tock' said at the beginning for a few beats. No strong sense of rhythm. It was a rhythm of 'sound,' 'silence.' There were strain-sensations during the sound, and relaxation during the silence. Visualised 'sound' as a space of one inch, and 'silence' of four inches from left to right. The edges of these were clean-cut. Sound was seen as a black line, silence as a long gap. Rather interesting. For a while realised that breathing corresponded: inspiration during sound, expiration during silence. Sounds were heard very slowly, with tremendous pause between them. [*Rate: 42 per min.*]

(Ge) Second beat started a one-two rhythm with accent on 'one.' The rhythm seemed to be more intense than before, accompanied by verbal counting. Gave up counting and watched for the peculiar beat (mentioned above), but counting still interfered with the perception of this peculiar beat. It was lost again, but later found to be recurring every sixth beat. It seems to be higher in pitch, of less volume, less intensive (?). Idea occurred where this could come from. Bell-arrangements [of some metronomes—not the actual case with the one used in the experiment] were suggested; then gave it up. [*Rate: 152 per min.*]

(Ge) After a few strokes of no rhythm, a 'one-two' rhythm practically without verbal counting and consisting of sounds only. The accent was on 'two.' Then a few groups of 'one-two' rhythm with accent on 'one.' This was reënforced by verbal counting in terms of kinaesthetic-articulatory sensations. Then confusion followed with no rhythm. Then a 'one-two' rhythm with accent on 'two.' Doubtful whether this lasted until the end. Started to be unpleasant, less unpleasant later, and indifferent toward the end. Required vague organic adjustment, preceded by unpleasant organic disturbance which gradually wore off. [*Rate: 92 per min.*]

As regards some of these introspections, allowance has to be made for the elementary stage of the experiment at the time. In general, however, certain facts already begin to show. Kinaesthesis is prominent; but it may be kinaesthesis of movements of limbs or head, of gross bodily movements, of respiration, of vague organic disturbances in chest and abdomen, or of articulation. Again, in many instances, kinaesthesis may be accompanied or entirely replaced by a series of visual images of movement. Finally, there may occur visual images of a purely symbolic character, in themselves sufficient for carrying the rhythm. Occasionally, and, so far, very rarely indeed, a purely auditory rhythm, without visual or kinaesthetic accompaniments, is reported. In almost every case of reported head, limb, or respiratory movement

made by *O, E* was able to verify the report by actual observation of the movement. As far as *E* could not, there was also complete physical relaxation.

Under instruction *A1 b,* which was directed to the introspection of 'accent,' we have the following reports:

(B) Kinaesthetic sensation (or image) of slight nod of head on accent. . . . Deep kinaesthetic or organic sensation in head (like a quick pressure) accompanied by a visual image of something in head pressing downward and frontward toward the mouth. (This may be the explanation of the nod I mentioned above.) Perhaps the kinaesthetic and organic sensations are intensified or else changed at the accent; may be only a change in breathing.

[*Rate: 92 per min.*]

(Bu) Accent is marked by left foot being put down. . . . Kinaesthetic sensation from left foot, down with accent, up with non-accent. Organic sensations in head and trunk of moving in time with the rhythm. These sensations are more intense on the accent and less intense on the non-accent. . . . Accent felt more on the left side of the body. Down movement with the accent.

[*Rate: 92 per min.*]

(D) With inspiration came the accent. Accent means strain; non-accent, relaxation. *Spannungsgefühl* less pleasant than *Lösungsgefühl,* but both are by no means unpleasant. . . . Feeling of strain that was continuous through the series but strongest on the accented beat. . . . Accented beats seemed louder, . . . sixth beat louder and visually sharper. There was a long pause after the accented beat. [Note: a description of a visual pattern of figures symbolising the beats follows.] . . . Duration of the beats about the same. Intensity very much the same, though intensity is more marked on the accent. . . . Every accented beat is marked by kinaesthetic sensations of strain in chest (breathing inhibited). There is relaxation for the non-accent. Auditory sensations of accent more intense. Localised accented beats higher up (in kinaesthetic terms of eye-movement). Kinaesthetic sensations in throat more intense and sometimes higher in pitch for accent.

[*Rate: 152 per min.*]

(Ge) Greater clearness for accented sound. Greater intensity and extensity of articulatory-kinaesthetic sensations accompanying counting. I am uncertain whether greater clearness involves greater subjective intensity of sound. . . . I am surer now about difference in clearness: there is a greater clearness and a longer ring, with a higher pitch, like G—GABC. [*Rate: 92 per min.*]

(Ge) A 'one-two' rhythm with accent on 'two.' Second sound differed from first in having greater clearness and in having a peculiar qualitative difference at the end. It seems to last longr. A certain determination was set up to attend to accented sound—I can't get away from it. Felt tongue moving quite markedly extensively and intensively on accented 'two,' while 'one' is articulated more easily (by opening of the mouth). The tongue in saying 'two' seems to interrupt the even breath coming out of the back of the mouth. The verbal 'two' was accented partly by the effort of change involved in the articulation of 'two.' These two different factors are not simultaneous, but sometimes accent is carried on by the auditory differences, and sometimes by the motor differences.

[*Rate: 92 per min.*]

We may summarise these results by pointing out that (1) there is usually strain for the accent; (2) the kinaesthetic sensations are, for the most part, intensified and, without exception, changed in form or complexity. Especially is this true of the sensations of respiration. (3) Where there is visual imagery, this also changes. Introspective facts reported by one or more *O*'s, but not by all, are: greater clearness for accent, higher pitch, changes in duration, in length of pause after accent, greater intensity of sound. Three points are certainly clear: (1) the kinaesthetic complex changes for accent and non-accent, (2) kinaesthesis on the accent is more intensive and is felt as strain or tension, while kinaesthesis on the non-accent is less intensive and is felt as relaxation, and (3) kinaesthesis, prominent as it is, may be temporarily or entirely replaced by visual or auditory complexes.

Passing on from subjective to objective rhythmisation, we obtained the following introspections from the general instruction *A 2 a:*

(Bu) Rhythm: 1, 2″ [*as given; rate, 152*].[48] Kinaesthesis of moving foot up, with non-accent; down, with accent. Kinaesthetic sensations of head-movement up, with non-accent; down, with accent. Pleasant affection. 2 much longer than 1, and seemed higher in pitch. Heard tune in the rhythm. Came readily and underwent no change.

(Bu) Rhythm: 1″, 2, 3′ [*given: 1″, 2, 3; rate, 200*]. Moved hand up, and then down with accent, in terms of kinaesthetic sensations. Pleasant affection. No change in accent after the first three or four measures—at first it was on 2. First measure undecided.

(D) Rhythm: 1″, 2, 3, 4, 5 [*given: 1, 2, 3″; rate 200*]. 1 was as long, approximately, as the sum of the durations of 2, 3, 4, and 5, but I am not sure about this. At first strain for the accented and relaxation for all of the unaccented. Kinaesthesis of nodding of head with the accent. Then, in addition, strain-sensation for the four unaccented (2, 3, 4, 5), relaxation for 1. 1 became shorter then (staccato). Counted only once (one measure) to make sure how many there were in it. No tendency to continue afterwards. Not easily gotten; not pleasant but interesting.

(D) Rhythm: 1″, 2, 3, 4, 5 [*as given; rate, 152*]. 1 more intense, clearer, and lasted longer. Later, 4 became accented, lasted longer, and became more intense than 2, 3, and 5. Kinaesthesis of counting to get rhythm which was hard to get and not securely held. More difficult to attend to it than to the rhythm in the preceding experiment [which was a 1″, 2, 3 rhythm]. Not very pleasant.

(Ge) Rhythm: 1″, 2, 3 [*given: 1, 2″, 3; rate, 176*]. First few strokes unrhythmical; caught in second measure. Once thought it was 1″, 2″, 3. Verbal idea, 'perhaps a slip in raising the cover.' Qualitative difference between accented and unaccented. Not merely a difference in intensity. 'Brighter' [mentioned in a previous introspection as a characteristic of the accented beat] now interpreted as

[48] Note: ″ means major accent; ′ means minor accent.

meaning 'higher pitch.' Unaccented more drawn out. Accented stroke sharp and definite—unaccented lost themselves temporally. Pleasant, 'of-course' rhythm.

(Ge) Rhythm: 1, 2" [*as given; rate, 176*]. Found at once and kept throughout, mostly as an auditory rhythm, except at the beginning where there was slight verbal counting. Attention very strongly on accent which seemed longer, higher pitched, and more intense than the lower, muffled, and briefer unaccented. Slightly pleasant, with 'familiarity tag.'

In quoting these introspections, in order to allow for practice and accommodation under the instruction, we have followed the plan of selecting one introspection of each *O* from about the middle and one from near the close of the series. The fact which the introspections, taken together with the complete set from which they are abstracted, brings out is that objective accent is, as a rule, of longer duration, more intense, higher pitched, and somewhat clearer than the unaccented beat. A numerical count of the instances of actual mention of these points in the reports is plainly deceptive because, while a characteristic may be noticed, yet very often, owing to the recency of a previous mention or to the unimportant nature of the appearance, no specific account will be given. To support our statement of the facts, however, and with the above reservation, the following table is appended:

	No. of times accented beat reported			
	Longer	More intensive	Higher	Clearer
Bu.....	16	1	3	0
D.....	15	9	1	2
Ge.....	4	5	7	2

In addition to these reported differences between accented and unaccented members, there were introspections on the kinaesthetic, visual, and auditory complexes mentioned in the first series, under the influence of subjective rhythmisation. They did not, however, appear so frequently, nor did they play so prominent a part. This may be due to the fact that the instruction was not focused on this problem.

Our next instruction called for an observation of the changes in the course of consciousness (*A2b*), and gave us results that were especially valuable. During the remainder of the series of experiments, it was this problem, *i. e.*, what are the significant *changes* in consciousness, casually hinted at in the beginning, that occupied a good deal of our attention. The problem is specifically suggested, for example, in such introspections as the following:

(Bu) I counted for the first few measures only. [*1, 2, 3, 4"; rate, 152*].

(D) Feel that head must be nodded when rhythm is difficult. [*1, 2, 3″; rate, 200*].
The nodding of the head helped to decide what the rhythm was at the beginning. [*1″, 2; rate, 176*].
Kinaesthesis of nodding of head, done to fix rhythm. [*1, 2″, 3; rate, 200*].
(Ge) Second and third measures counted; articulation stopped after this. [*1″, 2, 3, 4, 5; rate, 176*].
Mostly auditory rhythm—there was no necessity for verbal accompaniment except in two doubtful places. [*1, 2″; rate, 176*].

In accordance with this instruction (*A 2 b*), we obtained the following results:

(Bu) Rhythm: 1″, 2, 3′, 4 [*given: 1″, 2, 3, 4; rate, 200*]. Easily gotten. I did not count after the rhythm was decided. This was done in the first few measures. I thought of jog-trot in time to the rhythm (in terms of kinaesthesis of my own body). Visual imagery of the cadets in double-quick time. Auditory imagery of the band music. All of this occurred toward the end. Pleasant affection.
(Bu) Rhythm: 1″, 2, 3 [*as given; rate, 152*]. When I got it in the second measure, I stopped counting. Auditory imagery of one of the melodies of Easter church songs. Visual imagery, in about the middle of the series, of a minuet-dance. At the very end, kinaesthesis of entering into the minuet. Pleasant affection.

As a rule, Bu has kinaesthesis of nodding of head and of articulatory movements throughout the period, but in two or three reports, she notices, as in the above, that this ceased when the rhythm was certain. In such cases, an irrelevant sort of auditory or visual complex seems to take the place of kinaesthesis. There may be, however, a vague kinaesthesis to accompany this new complex. D is more definite on this point, and, in the large majority of introspections, reports a shading off of the kinaesthetic factor, especially in the capacity in which it occurred at the beginning of the period:

(D) Rhythm: 1, 2″ [*as given; rate, 152*]. . . . Kinaesthesis marked until near the end when the rhythm began to take care of itself. I did not attend to it with so much effort.
(D) Rhythm: 1″, 2 [*given: 1, 2″; rate, 176*]. This lasted for about two measures. 1 was longer and more intensive. Then a 1′, 2″, 3, 4 rhythm accompanied by movements of the head—left with first two beats, right with last two. Head really moved for the accent of the first beat. Then there was a plain 1″, 2 rhythm with no kinaesthesis. Rhythm was soothing. I felt drowsy.
(D) Rhythm: 1″, 2, 3 (?), 4 [*given: 1, 2, 3, 4″; rate, 176*]. Rhythm was troublesome at first. Kinaesthesis of jerking of the head to get it, also of counting. Then kinaesthesis dropped out, but I am not positive about this. There was relaxation for the unaccented (last three beats), but this changed to slight feeling of strain, due, perhaps, to slight adjustments of breathing. Affection indifferent. Time seemed to be quickened at the end.

Often a general bodily strain or tension is reported by D as being very prominent at the beginning of the period and as wearing away at the end. The intensity and the pitch of the accented beat, when they are carried kinaesthetically, diminish also. Ge frequently notices that the rhythm becomes merely auditory, with an attitude toward it that is called 'passive:'

(Ge) Rhythm: 1, 2, 3, 4″ [*as given; rate, 152*]. Counted once. Not a particularly easy rhythm. At the beginning it was accompanied by kinaesthetic sensations of movements particularly in the back of the mouth and partly in the eye-wink. Then there was a passive attitude of merely listening with the expectation of the accent and with the filling in of the beats. More of a time-rhythm. Time during the expectation period (during the unaccented beats) is felt as strain. Affection somewhat pleasant, straining. The rhythm was caught indifferently.

Occasionally there is also an illusion of an increase in the speed of the rhythm toward the end of the period. This illusion is also reported by D. With Ge it is evident that when a rhythm is simple and not difficult, the auditory nature of the rhythmical perception, and the passivity of attention as mentioned, are characteristic of the entire period:

(Ge) Rhythm: 1″, 2″, 3, 4 [*as given; rate, 152*]. Very easy and very comfortable rhythm. Passively taken in in auditory terms, except occasional tendency to move parts of the body (especially the right side) in a 1, 2 rhythm—one way with the two accented beats, and the other way with the two unaccented beats. This is an indication of movement, a translation into space perception of that which was not in itself spatial. It seemed to be more of the nature of kinaesthetic ideation of muscular contraction with the two accented, followed by kinaesthetic ideation of relaxation with the two unaccented beats.

As the result of the 'method of fractionation' after long practice and with instructions *A2 c* and *A2 d* arranged in haphazard order, we obtained the following introspections of the first period:

(Bu) Conscious attitude of hesitation during first measure; with the second measure came the verbal idea, 'I must get this.' Then articulatory sensation of counting for two measures. I decided that the second of these was right. Caught it certainly about the fourth measure. Feeling of hesitation and doubt disappeared. Pleasant affection instead. I ceased counting; attitude of satisfaction. [*Rhythm: 1″, 2, 3″, 4, 5; rate, 200*].

(D) Auditory sensations of successive sounds of varying intensity. 'Muddled feeling' in terms of organic sensations and 'mixed-up' kinaesthesis. Feeling of strain, chiefly in head. Then out of the muddle of kinaesthesis, I distinguished definite kinaesthesis in throat of definite sequence and intensity. This gave the rhythm. Then 'muddled feeling' went. Affection more pleasant, kinaesthesis less prominent. [*Rhythm: as given above in* Bu.]

(Ge) Great confusion. Succession of sounds for a long while. Difference in intensity of sounds noticed. Unsuccessful attempts to count, until one accent appeared and was called 'one.' Every time that accent came it was called 'one.' Strong organic background with unpleasant affection, and involuntary inhibition of breathing (this constitutes part of 'confusion'). Breathing resumed after successful counting started. Somehow, but I don't know how, first few strokes seemed to form a visual series (but not of visual images) into which strokes were translated. It was as if kinematic pictures passed too fast to be caught, except their motion. *Bewusstseinslage* of movement present. This may have been due to eye-movement; but eyes were closed. It was more like the trembling of the eye-lids. Localised visual movement in eyes.

[*Rhythm: 1", 2", 3, 4", 5; rate, 176*].

Introspections descriptive of the second period:

(Bu) Kinaesthetic sensation of moving forward with a sort of swaying movement. Left foot put down with accent. Articulatory sensations of counting. Verbal idea meaning, 'I must forget the rhythm and cease counting.' Visual imagery of people marching in parade. Pleasant affection. Auditory imagery of music with tune to suit the rhythm, accentuating with swaying motion.

[*Rhythm: 1", 2; rate, 152*].

(D) Mildly pleasant feeling of familiarity dying away after a while. Total relaxation, almost drowsy. Auditory sensations forced themselves on me. Attention passive. Rhythm gradually became unpleasant. Painful sensations in ear-drums becoming more intense toward end. Organic sensations and 'mussed-up' breathing. Rhythm decidedly objective. ['Objective' has been described by D as meaning lack of kinaesthesis that carries the rhythm, and also as meaning that the 'rhythm was definitely localised somewhere outside of me']. Attitude of resentment toward it. [*Rhythm: 1", 2; rate, 200*].

(Ge) Rhythm became clear as soon as verbalised with the accent. It was verbalised, in terms of articulatory sensations of tongue-movements, until the end, but became less intense, easier, and less extensive. Affection changed from unpleasant in fore-period to slightly pleasant in this period. Very concentrated attention on auditory rhythm. This was accompanied or marked by pressure of eye-lids against eye-ball, more particularly at beginning, but as a background to the whole. This was less marked at the end.

[*Rhythm: 1", 2", 3, 4", 5; rate, 200*].

In answer to the question (*A2 e*) concerning the difference between the two periods, the following statements were given:

(Bu) I am sure there is a difference. My whole attention is taken up with the desire to catch the rhythm (in the first period) and I have only those sensations which help me make that decision, *i.e.*, various kinaesthetic and articulatory sensations; whereas in the other case (in the second period) there is an attitude of satisfaction and verbal ideas meaning 'I can think of whatever I want to.'

(D) I should say that kinaesthesis was decidedly more prominent in the first period. In the first period the affective course is the reverse of that in the second period, *e.g.*, first period, unpleasantness changes to pleasantness; second period, pleasantness changes more gradually to indifference or even unpleasantness with organic sensa-

tions to correspond. There was more effort at attention during the first period. Attention more passive in second period. Auditory sensations are more prominent in the second period. The first period is more interesting and the experience more subjective with the feeling of bodily participation through means of organic and kinaesthetic sensations. There is a feeling of familiarity which comes just at the time of recognition and lasts a little afterwards.

(Ge) Rhythm-consciousness under the second instruction becomes more automatic, which term means a dropping-out of counting, a decrease in clearness, and a wearing-off of the affective tone. The rhythm-consciousness of the first instruction is somewhat influenced by the consciousness of the previous period—a lapping over of the consciousness of the previous period into the rhythmical consciousness (perception of sound). The difference between the two consciousnesses is a gradual change. Under the second instruction, I do not know when to begin to introspect, while under the first instruction I do not know where to stop. The main difference is one of degree of complexity and also, to some extent, a gradual change in the attributes of the conscious processes. [*Question:* Which consciousness is the more complex? *Answer:*] The first one is more complex. The attributes of the mental processes are at a higher level, *e.g.*, at first they are clearer, more intensive, more extensive, and more pleasant, and they gradually change from more to less. The first consciousness is richer in contents and the contents are of higher degree. The second consciousness is poorer in contents and the contents are of lesser degree. The second consciousness is more automatic.

The results of these preliminary experiments corroborate the above description taken in a *post mortem* manner. They may be summarised by the characterisation of a typical rhythmical consciousness as present under these experimental conditions. We must, however, exercise the greatest care in not giving too much prominence to this 'typical rhythmical consciousness.' It is not factual, but descriptive of a class of rhythmical consciousnesses; as such a class-description it must suffer all the qualifications that obtain under the type of generalisation by the method of approximation. We may say, then, that usually at the beginning there is confusion of some sort, accompanied by a complex of mixed kinaesthetic, and, occasionally, visual and auditory sensations, and by unpleasant affection. The contents of consciousness is full, but it is not orderly. There is little or no pattern. Soon,—and the rapidity with which this change takes place seems to depend, among other things, upon the degree of difficulty of the rhythm and upon the mental set and the physical condition of *O*,—some phase of kinaesthesis becomes prominent and is definitely localised and recognised in the introspection. More rarely, some series of visual imagery comes into the foreground. If these processes come into the focus of attention individually, they are usually followed by a combination of several processes, all more or less equally prominent, and all

well-ordered and recognised as forming a pattern. Attention is, on the whole, of a high level and discloses a consciousness rich in content. A feeling of strain characterises this high level of attention, and the rhythm becomes definitely fixed in perception. A feeling of satisfaction now ensues, and the affection is reported as pleasant. Should the rhythm be more than moderately difficult, and should it, therefore, not become definitely fixed, or should the mental set of *O* be such that he can not make the rhythm 'fit in,' then pleasant affection may never be reported, and strain-sensations may continue in a vague degree until the end. This was actually the case with some of the odd and infrequently heard rhythms used in the experiments. Then, ordinarily, sensations of strain gradually die away, attention drops in level, kinaesthesis grows less intensive and extensive, and finally vanishes completely or becomes irrelevant to the rhythm. The rhythm is heard merely in terms of auditory perceptions. All kinds of associative ideas may now come into consciousness, but, in the experiments with the 'degeneration' of the rhythmical consciousness, they, too, disappeared. Affection changes gradually from pleasantness to indifference and sometimes to unpleasantness. The rhythm is occasionally reported as 'boresome.' At the extreme duration of one minute in the 'degeneration'-experiments, we found a consciousness almost barren of content, characterised as 'drowsy' and 'sleepy.' The introspections were, as a rule, very brief. As a matter of fact the *O*'s were about ready to fall asleep and often said so. Indeed, consciousness strongly resembled that in the preliminary stages of hypnosis.

This description is based upon facts collected from the preliminary reports of the *O*'s. The evidence is clear and definite, and as far as the typical description goes, it is free from conflicting statements in the introspections of the several *O*'s. But, owing to the preliminary character of the series, the inadequately controlled physical conditions, and the lack of training on the part of the *O*'s, the results so far obtained demanded more substantial corroboration. For this reason we isolated the physical attributes of tonal stimuli as duration, intensity, and pitch; we trained our *O*'s by means of a long series of judgments and introspections; and we shaped our instructions to cover the special points of attack.

B. Main Series.—The following are extracts from the reports of *O*'s under the first instruction in the series (*B*1 *a*):

(D) Trochaic rhythm as before. It began at once but did not seem so markedly a rhythm as before. Breathing (inspiration for long, expiration for short) corresponded. This was my natural rate,

therefore it did not help the rhythm materially. Kinaesthesis of counting 1 . . . 8 or more at the beginning to help the rhythm—I believe to give it more body. Rather monotonous affair. At about the 8th group, noticed a tonal difference and began saying 're, ti,' *etc.* Verbal idea, with kinaesthesis giving it meaning, 'All this is rather aloof from me,' *i.e.,* from the auditory part of my experience. Kinaesthesis was subjective; auditory sensations localised outside, to the right. Some kinaesthesis of eye-movement (?) which corresponded to the duration of the members.

[*Rhythm:* .5", .05"].

Measures not sharply marked off from one another. In my schema there was no break. Schema: kinaesthetic sensations from eyes which would go as visual schema, but it was not seen as such. Some kinaesthesis in throat of counting 1 . . . 8 (and on ?) with overlapping 're, ti.' Then slight head movement, up for high, down for low, accompanied this. [*Rhythm:* .5", .05"].

Distance between members of a rhythm not as long as that between measures. Recognised this as objectively true because of lack of exact correspondence with breathing. Tendency to hurry up one phase of breathing and retard the other. This was slightly bothersome. [*Rhythm:* .4", .2"].

Slight tendency of visualisation of two members as flat thick lines with short distance between them and an implication of a long distance on either side. Lines were seen only two at a time. Kinaesthesis of this more marked than visualisation. Fixation in front. Feeling of balance for the two halves of the measures conveyed by equal intensity and duration of the members and the similarity of the kinaesthesis for the two, although the first was visualised to the left and the other to the right; kinaesthesis was directed correspondingly. Not very marked kinaesthesis located in upper part of trunk, a little throat-kinaesthesis, vague and indefinite.

[*Rhythm:* .05", .5"].

Attentive attitude and fixation as before. Qualitative difference in pitch—first higher. Throat-kinaesthesis and auditory imagery corresponding to the quality of the tone. Another long interval and a repetition of the same. Then experience began to be rhythmical—although, perhaps, made so by subjective experience: breathing with an effort to bring the two together. There was also vague kinaesthesis, organic sensations, and some eye-movement (which was important). Toward end there was a trochaic rhythm with very long intervals—ready to break apart at the slightest provocation. Experience slightly unpleasant—so much of a bother—I was set to get the rhythm at all costs. [*Rhythm:* .45", .1"].

(E) Auditory sensations—not very clearly perceived at first. Following first auditory sensation there was a conscious complex which I do not analyse [analysed later: there was doubt in regard to rhythm which was carried by kinaesthetic sensations located (?) in neck, body, and limbs with slight contraction], but which carried the meaning: 'what is the rhythm,' or 'I want to know what it is.' At about this time kinaesthetic sensations located in neck, back, and head, from nodding with the rhythm. Also at this time, or immediately following, kinaesthetic sensations in throat, tongue, and lips as if from incipient singing of tones. After-images from tones, very vague, if at all; memory-image of each couple held over from end of one stimulus to the next. [*Rhythm:* .5", .05"].

At about 3rd group, perception of rhythm, accompanied by pleasant affection and ideas meaning: 'I have caught the rhythm,' 'it goes easier now,' and with following groups as they came there was a perception of the rhythm and recognition of each following group as being like the 3rd group just recognised. [*Rhythm: .5", .05"*].

Attention good throughout the series and tones were in the focus of consciousness. All other contents were much more marginal. There was one fluctuation—this occurring in the interval between the 2nd and the 3rd group, and in this interval there appeared for a moment, very near to the focus of consciousness, kinaesthetic sensations (including general bodily sensations, mentioned above) and perception of comfortableness (not analysed).

[*Rhythm: .25", .25"*].

Both were equally long; this came with the second sound and the perception of the rhythm, and was carried in terms of kinaesthetic sensations which were rather unpleasant. There seemed to be some slight strain-sensations among these. At the same time came ideas (kinaesthetically carried?) which conveyed the meaning, not definitely outlined or worded: 'these tones are fainter and lower in pitch and I can't reproduce them myself as easily as I could the others.' Slight interval. Perception of second group. Perception that these were slightly more intense—due to kinaesthetic sensations from ear. Perception that these sensations formed a group like the first. Unpleasant affection. Unanalysed complex—surprise, vexation, and hesitation. During this time slight strain-sensations in chest, neck, and throat aroused by changed breathing and attempting to reproduce tones internally. Kinaesthetic sensations were quite prominent throughout the period. Toward the end, idea, carried verbal-kinaesthetically, 'I don't like this so well.' [*Rhythm: .25", .25"*].

(G) Knowledge that this sound is the same in intensity, pitch, clearness, and duration as that of the first member. Then same kinaesthesis of muscular tension. Organic sensations pleasantly toned. Pressure sensations of position. Organics. Pressure sensations and sensations of muscular tension were always blended throughout but there were different degrees of blending. Interval following was longer—carried organically. All of this repeated to the end. After the second member of the first group, visual imagery came into consciousness, represented by: [here figure was drawn showing series of black dots, growing more heavy and thick as series progressed, and arranged in a line ending with the heaviest dots to the right]. Line tilted at an angle greater than 90° to the plane of vision. These dots were light and the background black.

[*Rhythm: .25", .25"*].

Auditory sensations of first member in focus. Sensations of general muscular tension. Strain-sensations in left ear and head. Organic sensations very slightly pleasantly toned. Pressure-sensations, *etc.*, of my position. Auditory sensations (or imagery?) faded away and sensations of muscular tension, organics, and pressure-sensations present during interval. Consciousness was at a lower level. Auditory sensations of second member, and knowledge that this was longer, clearer, and more intense (pitch: higher ?), carried in organic sensations. This was in focus. Sensations of general muscular tension. Strain-sensations in ear and head. Organic sensations pleasantly toned (slightly different from those corresponding to first member ?). Pressure sensations of my position. Interval, during which strain, pressure, and organic sensations were present

(longer than the interval between members, carried organically ?). All of these processes repeated exactly in the same way to the end. [*Rhythm: .35", .15"*].

We already begin to note the prominence of kinaesthetic factors in the rhythmical perception, but in the reports of both D and E most of these factors have to do with the initial perception, the 'getting,' of the rhythm, and give place to purely auditory patterns when the 'fitting-in' of the rhythm, or what D describes as giving it 'body,' has been accomplished. G, who is apparently more organic in type, has the kinaesthetic factor present in varying degrees throughout the period; but she, too, distinguishes between the general muscular components and the strain sensations which shift about during the period and undergo various 'degrees of blending.' It is difficult to tell what reference all these changes in the texture of consciousness bear for her, because she seldom reports 'meaning;' yet, from a consideration of her descriptive statements, it is evident that kinaesthesis is most often connected with a judging attitude, *i. e.*, with the estimation of durational, intensive, and qualitative peculiarities of the tones and their intervals. We find, however, with all the prominence given to kinaesthesis, that other complexes come into consciousness, especially near the end of the period when the rhythm has been 'caught.' All of the *O*'s report visual imagery; some report pure auditory complexes without kinaesthetic accompaniments.

Since it was the aim of the series to present a purely durational rhythm, and since from the introspections given it was clear that this object had not been attained on the conscious side in that all of the *O*'s reported pitch and intensity, a series of instructions, *B* 1 *b*, and *c*, were given in order to cancel this tendency on the part of the *O*'s. We find, then, that when the instructions, which called for a fractionation of consciousness into three periods, were given to the *O*'s, there was a much greater uniformity of report on the durational factor alone.

If we consider first the reports given for the period lasting from the 'ready' signal to the perception of the first sound, we find again that kinaesthesis centers around the instruction:

(D) Kinaesthesis in head particularly around eyes which meant a vague realisation of *Aufgabe*. Bodily posture more rigid and strained than times without this *Aufgabe*. Eyes turned toward right ear—attitude meaning expected sound coming from that direction. Breathing slightly strained—result of a new *Aufgabe*. Affective tone perhaps slightly unpleasant—meaning bother with new *Aufgabe*. [*Rhythm: .08", .34"*].

Rather strained bodily posture. Strain-sensations in trunk and top of head. Eyes turned to right. Kinaesthesis around them meaning: vaguest flicker of realisation of *Aufgabe*. Vague visual imagery of apparatus at my ear. Affective tone rather unpleasant. Trunk-kinaesthetic strains increased. Then sounds came in terms of auditory perceptions. [*Rhythm: .3", .1"*].

(E) Cutaneous sensations from pressing button. Then dark visual background which was large and into which there came slowly visual imagery of speaking-tube. Then visual imagery of table changing into visual imagery of other room and *E* seated. Next, conscious complex, mostly kinaesthesis, meaning: *E* is giving me a fairly long interval before the tones. Instructions appeared dimly in kinaesthesis. Perception of what I had to do—understanding of it—carried in kinaesthesis. [*Rhythm: .3", .1"*].

Tactual after-image from pressing button. Visual after-image from light—these faded. Dark visual background. General bodily sensations in chest and different parts of body from pressure of clothing, chair, *etc.* Visual imagery of *E*'s room, *E*, and apparatus. Some expectation carried probably entirely in kinaesthesis. Slight strain-sensations which came from attending to (listening for) sounds. [*Rhythm: .22", .1"*].

(G) Sensations of general muscular tension. Respiratory sensations—holding breath slightly. Strain-sensations in ears and head. Sometimes vague visual images of 'pitch,' and 'intensity' (as if written), and something else which stood for 'duration' (?). All these formed an expectant attitude of what I was going to do, *i.e.*, the *Aufgabe*. At very lowest level of clearness—just barely conscious—a confused mass of organic sensations—all 'muddled up.' All processes above mentioned were present during the entire period—excepting visual imagery which came only a few times. They were, however, varying in degrees of clearness, *e.g.*, strain-sensations in ears were very clear—sometimes sensations of general muscular tension. [*Rhythm: .3", .1"*].

In comparison with this sort of description, we find that a report of the second period, from the perception of the first sound to the perception of the rhythm (instruction *B 1 c*), shows a different pattern of consciousness:

(D) Was set for a two-membered rhythm (I infer this). Sounds localised to right and in front—to right of right ear. Bodily posture quite rigid and alert. At first sound there occurred a certain complex of kinaesthesis (especially around eyes) which meant anticipation of rhythm as trochaic. This anticipation was in terms of estimation of particular amount and intensity of strain which accompanied auditory sensations. Then came second sound which confirmed my decision—confirmation in terms of relative amount and intensity of kinaesthesis. Then attitude of assurance—in terms of deeper, freer breathing, tendency to expand chest and to hold head straighter. Rather pleasant. [*Rhythm: .26", .14"*].

A lot of eye-movement with corresponding sensations in neighborhood of eyes which meant a comparison of sounds for duration in terms of eye-strain. A comparison, too, of throat-kinaesthesis which accompanied sounds. Experience quite unpleasant accompanying sensations of nausea and tightness in the diaphragm region. Verbal idea, after a while: 'of course, that was a trochaic rhythm.' Then

verbal idea: 'you have only compared the duration.' Then more eye-movement which meant a comparison of intensities of kinaesthesis. [*Rhythm: .22", .22"*].

(E) Perception of first group. Slight strain-sensations. Feeling of doubt carried probably by strain-sensations. Perception of second group. Recognition of them as a kind that had been given before. Then judgment followed in words 'short, long.' Next group, words repeated in internal speech. Judgment 'equal' was made for intensity and for pitch—came by kinaesthetic sensations especially in neck and head (nod of head) which were equal.
[*Rhythm: .22", .22"*].

Perception of first member, second following, and then perception of group. First was perceived. Then some unanalysed background processes which I do not now analyse. Perception of second with perception of each being equal in pitch and in intensity. Judgments of these not given in the same way—not same processes in one judgment as in another: one, spoken in internal speech, another, kinaesthesis carrying the meaning. Experience of this experiment (*Aufgabe*) does not run easily yet. Strain-sensations in face and from body, especially from changed breathing involved in listening to tones and in trying to make judgments. [*Rhythm: .3", .1"*].

(G) Second longer in duration. When I passed my judgment I felt that if the second had been equal in duration, judgment of pitch would have been made more certain—carried in organic sensations. Also felt rather unpleasant when I felt uncertain about pitch and intensity—accompanied by strain sensations. Carriers of rhythm: kinaesthesis of body (in trunk) and respiratory sensations tending to keep time with rhythm. . . . Carriers of rhythm were always in upper level, but varied somewhat in degree of clearness with the exception of two or three times when I was trying to get a final judgment on intensity and pitch. These times I made a supreme effort. Then strain-sensations and sensations of general muscular tension characterise, and the carriers of the rhythm were out of consciousness. [*Rhythm: .04", .38"*].

In general, in this period, kinaesthesis clusters about the clear perception of the rhythm, the 'carriers of the rhythm,' and also about the judging complex. It has changed in meaning from that which it bore in the previous period, *viz.*, the comprehension of the *Aufgabe* and the expectation of the rhythm; it is often the same sort of kinaesthesis, connected with movements of the eyes, breathing, *etc.*, but it is recognised as referring to something else. Throat-kinaesthesis seems to play a prominent part in the judgment and realization of the rhythm, as well as general strain-sensations indefinitely localised or general throughout the body. In some cases visual imagery also plays a rôle.

In the last period, however, if the judgment has been properly passed, most of these kinaesthetic complexes relax, become less clear, and less intense. If, as in G's case, kinaesthesis persists, it may become irrelevant to the rhythm.

The entire pattern of consciousness becomes different while the rhythm is still present in perception:

(D) Throat-kinaesthesis at first. This soon died away. Auditory experience left to itself. Almost went to sleep. Interesting only at first. Pleasant throughout. [*Rhythm: .12", .3"*].

Bodily posture relaxed—no accompanying kinaesthesis. Affective tone indifferent or mildly pleasant; began to feel drowsy. After a while sounds became provoking—intruding upon a relaxed consciousness—unpleasant organic sensations. Strong inclination to shake head—to get them off (incipient kinaesthesis). Unpleasantness increased but intermittently so. At times the sounds did not bother so much as at other times. [*Rhythm: .08", .34"*].

Bodily posture relaxed. Throat-kinaesthesis much reduced, then died away gradually. Respiration ceased to correspond. Eye-kinaesthesis continued for a while much less intense. Began to have achy sensations in ears. Affective tone changed from indifferent to positively unpleasant. Increasing irritated feeling. Sounds seemed to force themselves upon me—I couldn't escape them. [*Rhythm: .08", .34"*].

(E) Clearest contents of consciousness were the auditory sensations, and there were also general bodily tactual sensations. Very soon a great number of visual images appeared—images of people around the laboratory and the rooms of the laboratory, especially this room [dark-room]. Clearness of auditory sensations changed from time to time—once or twice they became marginal. Visual imagery changed quite rapidly, and there appeared from time to time repetitions of judgments—they were now all made in terms of internal speech. There soon came a comfortable feeling, rather of an enjoyment of the situation. Strain-sensations prominent in attention became slightly less, especially sensations in face, over the eyes, and in chest,—some also in arm and hands. There seemed to be no visualising of tones as I have noticed before—all auditory. There were also from neck and head (from nodding of head). This meant that I was satisfied with the judgments I had made; that judgments were correct and that I need not attend quite so closely for the purpose of making a judgment. . . . [*Rhythm: .25", .14"*].

(G) Verbal idea: 'Now I have found out what the rhythm is; I will just give myself up to the thing.' [*Rhythm: .3", .1"*].

Throughout the experiment, auditory sensations from tones in receiver with weak sensations from breathing (I tried to keep time) *plus* kinaesthesis in trunk—locality abdominal and above abdomen—also pleasant. Accompanying these, during the first part of the period, fairly weak sensations of general muscular tension. These gradually got fainter and disappeared—their place being taken by general relaxation during which general bodily kinaesthesis became clearer and more intense, and those of keeping time also became clearer and more intense—I mean the sensations of breathing. [*Rhythm: .25", .14"*].

At first part of third period, sensations of general muscular tension, located principally in head and trunk—these gradually got fainter and less clear until they disappeared and a feeling of relaxation took their place until the end of the experiment. Feeling of relaxation became more intense—rather soothing. Then, auditory sensations of tone, and these were accompanied by general trunk kinaesthesis which was

pleasant. Kinaesthesis located principally in region of abdomen and above abdomen. This kinaesthesis, as the period progressed, became less clear and intense, and shifted. [*Rhythm: .12", .3"*].

G has, however, much more kinaesthesis accompanying the rhythm throughout the entire period than have the other *O*'s. While there is a marked shift in pattern of consciousness, with accompanying shifts in the relative intensity, clearness, and modality of the processes, and while there is a decided change in the meaning which kinaesthesis bears, *i. e.*, a tendency to mean 'relaxation,' a great deal of kinaesthesis from breathing survives and accompanies the rhythm. *E*, however, noticed that G was never as sure of her judgments as were the other *O*'s. If, therefore, a doubtful attitude was taken in this period, G never assumed a passivity of the sort experienced by all of the other *O*'s throughout the entire experiment. She says, for example:

Intensity, pitch, and duration equal. This settled fairly quickly, but continued to judge intensity until near the end—to make sure. [*Rhythm: .22", .22"*].

When the last period was lengthened to one minute, the facts above mentioned in connection with the observations were verified. That the results obtained in this 'fractionated' series are not rendered invalid by this method of 'fractionation,' can be assumed from the comparison of these results with those that were obtained when the period was not subdivided.[64] All of the results given are in agreement, with the possible exception of G's observations, for which a special hypothesis has been advanced.

To continue with the results obtained in series *B2*, in which a rhythm was produced in terms of physical differences of intensity, we find further confirmation of the above facts. Since these confirmations were obtained under new conditions, they may be quoted. Under the first general instruction *B2 a*, we get introspections such as the following:

(D) Acted under *Aufgabe* to judge duration first. Comparison of duration in terms of throat-kinaesthesis accompanying the two sounds, which was in turn measured in terms of eye-kinaesthesis, *i.e.*, throat-kinaesthesis accompanying each sound was accompanied by a certain amount of eye-movement which corresponded in length to the duration of the sound. There may have been slight visualisation of throat-kinaesthesis as two gray bands. Visual part very slight, chiefly in terms of eye-kinaesthesis. That for the second sound was longer

[64] The *O*'s were asked to give an estimate of the 'completeness' and 'accuracy' of their introspections on the basis of 100%. None of the estimates given were below 80% for either one of these; most of them ranged from 90%-100%.

than that for the first, therefore judgment that the second was longer. Judgment repeated itself three or four times automatically. Next judgment one of pitch. Two sounds were localised (visualised ?) in correspondence with their pitch. First sound localised above second—judged higher. Throat-kinaesthesis involved somewhat. Repetition of first two judgments 'longer and lower.' For third judgment, kinaesthesis of throat was measured for intensity. In correspondence with intensity of throat-kinaesthesis—visualisation of two sounds as rounded figures, first to the left of second. First seemed to stand out more (in third dimension toward me) but there wasn't very much difference between them. Finally made verbal decision. [*Rhythm: 1>2*].

(E) Perception of first tone and imagery present became dim, tone becoming clearer. Slight organic shock as attention turned from imagery to sensational complex. Tones perceived merely as tones for a moment or two—no judgments being made as to rhythm. Then suddenly words 'long, short' spoken in internal speech accompanying the realisation that these tones seemed to be very much like memories of other 'long, short' series. This was not a judgment. Judgment on intensity was made kinaesthetically—a stronger contraction, tension for first, less for second. About this time judgment as to duration was made [equal] merely in terms of slight nod of head which meant 'equal.' Some confusion like inhibition of judgments—strain-sensations were present. Finally pitch judged by a nod of the head—this carried judgment 'same.' Complex of bodily sensations throughout the experiment, partly from change in breathing.
[*Rhythm: 1<2*].

(G) Sometimes I just gave myself up to the rhythm; at these times, auditory sensations of tones, a kinaesthetic swing of the trunk with each auditory sensation, pleasantness, and a general muscular feeling meaning relaxation would be in a cross-section of consciousness. This 'giving myself to the rhythm' occurred at the first only: during the latter half I judged intensity, pitch, and duration. Then there were marked strain-sensations in ears, and a general feeling of strain,—rigidity. [*Rhythm: 1>2*].

In the series of fractionations which followed, *B2 d*, *e*, and *f*, we obtained such introspections as these for the first period:

(D) Assumed usual position, eyes turned toward direction of right ear. Expectation in terms of kinaesthesis of head and eyes that sound would come from that region. Otherwise performance quite automatic—no *Aufgabe*. My actions were all automatic.
[*Rhythm: 1>2*].

(G) Strain-sensations in region of ears, forehead, and eyes; kinaesthetic and pressure sensations from breathing, meaning *Aufgabe* in general and expectancy of the rhythm (part of the *Aufgabe*) in particular. [*Rhythm: 1>2*].

For the second period, we have:

(D) First auditory sensation compared in terms of kinaesthesis of head, eye, and throat. There were unpleasant organic sensations. Feeling of perplexity—this lasted quite a while—at same time verbal idea: 'I wonder if the first member is really longer or shorter than the second member.' This in terms of kinaesthetic attitude. Before this a perception of grouping in rhythm. Second member higher.

Idea, chiefly attitudinal, 'whether it seems longer because it is more intense.' Comparison of amount of strain accompanying each sound for duration. Sometimes second longer than the first; sometimes first longer. Final decision, probably equal in duration; second is equal or longer. Intensity, second weaker. Unpleasant coloring in terms of organic sensations throughout the period.

[*Rhythm:* $1>2$].

Characteristic of the last period is this description:

(E) Repetitions of judgments in terms of kinaesthetic processes, in nods very largely, not clearly in speech. Attention to tones not so good as before; general bodily sensations especially from breathing. Pleasant affective tone becoming clearer—seemed to occupy consciousness more fully with the tones. Occurrence of question in terms of verbal-kinaesthesis, 'Was I right?' Then a re-judging of the rhythm. Then assurance of the correctness of the judgment of the rhythm. Again attention much poorer on tones. Much pleasanter affective tone occurred with general bodily sensations. Sensations from change in breathing. Reappearance of *Aufgabe* inattentively in consciousness; words in internal speech in regard to judgment arose from time to time. At one time, visualisation of tones as spots to left—in front of me—one being more conspicuous than the other. Nothing else that is of any importance—nothing that I can recall. Consciousness not very rich, not nearly as rich as consciousness in previous period. [*Rhythm:* $1<2$].

These data further establish our conclusion. Kinaesthesis in the first part of the period carries the meaning of expectancy and the *Aufgabe;* consciousness is, however, not rich in processes, nor complex in pattern. When the rhythm is first heard, kinaesthetic strains, muscular contractions in head, throat, neck, and the other parts of the body, appear as references for the interpretation of the rhythm and as aids to its clear perception; consciousness is rich in processes of several modalities,—visual, auditory, kinaesthetic complexes come in. Gradually, a large part of these kinaesthetic processes disappear; a few may remain; some may become associated with the meaning of relaxation. Irrelevant imagery may appear, verbal ideas may arise; but, in general, consciousness again becomes relatively poor in contents, and less clear in attention. All of these facts were more emphatically brought out when the entire period was lengthened to one minute ($B2\,h$). We were interested in the question how much kinaesthesis would be present in the rhythmical consciousness when the condition of passivity was insisted upon by *Aufgabe*. For this reason we gave D instruction $B2\,g$. Under this instruction she says:

(D) Fixation and passive attitude as before. Deep, full breathing with some attention on that as well as on right ear—lazy feeling after a few seconds—then sounds came, localised in right ear. First one longer and louder than second—fairly smooth and rather musical.

Immediate acceptance of trochaic rhythm. Vague bit of kinaesthesis chiefly in head, in eye-region, somewhat in terms of respiration, *i.e.*, breathing became more easy after acceptance. There was involuntary throat-kinaesthesis accompanying each sound, really a repetition of each sound as it came. Part of time remained passive but after a while attention wandered from sounds in my ear to kinaesthesis in my throat. A comparison of throat-kinaesthesis accompanying sounds in terms of their intensity and also intensity of eye-kinaesthesis accompanying throat-kinaesthesis. Then verbal idea: 'you are not following *Aufgabe,* you should relax.' This was followed by readjustment of bodily posture, decrease of throat-kinaesthesis. Then later, verbal idea, chiefly attitudinal: 'the rhythm seems much more alive when there is throat-kinaesthesis. It seems rather monotonous without it.' Affection indifferent. [*Rhythm: 1>2*].

With a change in two of our three *O*'s, and with our apparatus altered to give differences in pitch in the objective sounds produced, we pass on to the next series of introspections. As the result of fractionating the entire period, we obtained characteristic introspections of the first period, of which the following are abstracts:

(B) Strong, clear, tactual-kinaesthetic sensations from hand and forearm (pushing button), overlapping temporally auditory sensations and ear-strain *plus* eye-movement sensations (mean sound of buzzer with slight effort to hear it). Followed by a considerable period characterised by kinaesthetic sensations in head, like a very faint dizziness *plus* slight eye-strains *plus* visual sensations of very dark, uniform, black field (means blankness, nothingness, a sort of unconscious expectancy). Followed by kinaesthesis in chest—strong, means: 'oh, I must listen for the rhythm.' Interval. Followed by auditory sensations *plus* visual imagery of *E plus* eye-strains (means *E* walking heavily on floor). Followed by sensations—conscious attitude, perhaps,—means: 'no, that isn't *E;* it's someone pounding.' Very fleeting. Followed by interval characterised by expectancy, impatience. I do not remember content. Quite unpleasant; somewhere in the interval I wondered whether anything had gone wrong and the rhythm wasn't going to come; also thought how long the period was; also wondered if I could ever report it all. These things came up with prominent kinaesthesis and a little visual imagery, generally accompanied by eye-strain. There may have been slight verbal tags. I cannot, however, remember the content in detail.
[*Rhythm: a#, d'*].

(F) Pressed button, turned eyes toward left ear. Strain-sensations begin and continue throughout the period. These strains are localised especially in abdomen, though somewhat less intense strains in chest, arms, neck, and face. The abdominal strains seem to color the entire experience slightly unpleasantly, especially after I have waited a little and the sound doesn't come. Very vague auditory imagery of a sound and auditory verbal imagery: 'pitch,' 'rhythm,' during the period.
[*Rhythm: d', a#*].

(G) Pressure and muscular sensations in right hand from pressing button. Then mass of kinaesthetic sensations from trunk, upper part principally, and at times taggy very unclear auditory-verbal kinaesthesis, all standing for the instruction. Then strain-sensations in

left ear-region and some other unanalysed organic sensations—forming expectant attitude of what was to come.

[*Rhythm: c', a#*].

For the second period, we have the following descriptions:

(B) Auditory perception of first member. Followed by strong kinaesthesis in arms, trunk, and head (means: adjustment to situation and effort to attend to sounds). I think above was accompanied by unclear kinaesthetic sensations in chest (meaning: expectancy of second member, sort of anticipation of the two-rhythm). Auditory perception of the second member, including strong kinaesthesis in body. The second perception came not suddenly but as if anticipated. I think the kinaesthetic factor was not new, but the old anticipatory kinaesthesis became clearer and probably intensified. The second member came almost as familiar, although I cannot say that the familiarity was carried by anything more than the kinaesthesis mentioned above, and the whole consciousness was more indifferently than pleasantly toned. After second member there was period of strong kinaesthesis, very different from that mentioned—meaning was uncertainty; I should say now that it was uncertainty as to whether I had yet perceived the rhythm or whether I should still go on.

[*Rhythm: a#, c'*].

(F) First two sounds which came to me formed a rhythmical foot this time. In period of waiting for sound to come, I had talked to myself, something like this: 'there is really a rhythm coming, maybe it will be like the last.' Turned eyes toward ear, strain-sensations, auditory images of rhythm to come ('high, low') as in last experiment. Now when sound did come it was high like the last, and I think I held my breath, or at least the strains in the upper part of my chest and face kept steady, as if I were expecting the second part of the rhythmical foot (steady strains in abdomen, chest, eyes, and face are signs of expectation of sound in the first period. 'Steady' does not mean steady in intensity and clearness, but in duration). Now when the first sound came I think the abdominal strains decreased or disappeared this time while the other strains of the fore-period did not till after I had at least gotten a first foot. [*Rhythm: d', a#*].

(G) Auditory sensation, then another auditory sensation. Accompanying the second, unanalysed organic sensations—principally in trunk, meaning slight surprise and a 'stutter.' Then auditory sensation of first member, then auditory sensation of second member, accompanied by unanalysed organic sensations meaning: 'not so much difference in pitch.' Then unanalysed sensations (organic) in trunk meaning: 'judge as soon as possible.' Again auditory sensations of first member and auditory sensations of second member, accompanied by unanalysed organic sensations meaning: 'second slightly higher in pitch.' [*Rhythm: a#, d'*].

The reports for the last period follow:

(B) Period was very long and I cannot more than indicate its general form—mostly in *Kundgabe*. . . . Later a very long period in which rhythm was in unclear consciousness. This was a period of waiting for the sound to cease. The prominent thing in it was eye-pressures and strains. Many clear processes of note-taking—*e.g.*, visual images of the eye-pressure, meaning: 'that's

what's in consciousness now.' Organic processes from all over the body would become clear at times and then fade away, leaving the eye-pressures again (these were not the usual eye-movement strains). I can describe the whole period as one of restlessness and impatience for the period to end, with growing unpleasant affective tone. . . . Later period in which I remembered that I probably ought to report pitch differences. Now high tone came into clear consciousness, clearer than low. I seemed to take it analytically in an attempt to describe it. It seemed particularly pervading and granular, rather compressed and thrusting—these are all really meanings of other processes in consciousness, principally visual imagery and ear-kinaesthesis. [*Rhythm: d', c'*].

(F) Attention after this wandered from sounds in receiver to other sounds. . . . I talked to myself: 'why doesn't he shut it off?' 'period is long,' *etc.*

[Note: F, left to himself in this period without any definite task, devises little problems and experiments with subjective shifting of the accent regardless of its objective fixation. In a number of introspections he describes this process in detail. In the prominence of kinaesthetic strains that are called forth with this effort, his consciousness thus described is very similar to one which E once reported when, under the instruction with full knowledge, he was told that a certain form of rhythm was forthcoming, though, because of a slip in *E*'s technique, its direct opposite was produced! Strain and other kinaesthetic sensations forced a subjective rhythm into consciousness by suggestion from the instruction, in direct opposition to a marked objective form.]

[*Rhythm: d', a#*].

(G) With each auditory sensation at first a slight kinaesthetic swing of trunk from left to right with a feeling of general relaxation in terms of general muscular sensations *plus* pleasantness. Kinaesthesis disappeared, relaxation grew more and more distinct, auditory sensations less clear. Then a mass of organic sensations (unanalysed), meaning: 'suppose I must judge duration and intensity.' Then a mass of muscular sensations *plus* unpleasantness, meaning: 'effort to judge.' Judgments carried by unanalysed organic sensations. After judgments, visualised 'duration.' Then relaxation gets more and more distinct again; pleasantness present; auditory sensations less distinct also—they had gotten more distinct during judging. [*Rhythm: a#, d'*].

Several *O*'s had frequently referred to sensations of respiration in their observation; D had repeatedly noted that her respiration kept time with the rhythm; E, F, and G are conscious of breathing sensations when they are judging the rhythm. It is, in general, inconceivable that the rate of breathing could have corresponded with the rhythm as it was reported, *i. e.*, in its simple form, because a reference to the account of the apparatus will show that the rhythmical cycle was uniformly executed in 2 sec., while the average rate of breathing of the *O*'s tested was about 3.75 sec. for the complete cycle: no *O* ever breathed more slowly than one cycle in 5.7 sec. or faster than one cycle in 2.7 sec. That respira-

tory sensations carried meanings is factual, and, from the theoretical side, plausible. The curves for all the *O*'s tested show a retardation of rate in the period before the rhythm is heard, but no marked variation from the normal rate after that; *i.e.*, there is no uniform change in the rate at any definite place in the period outside of the one described, and the variations during the entire period are not noticeably different from the variations in the corresponding normal curve. Instances to illustrate this fact, taken at random from typical curves, are: a normal curve on a given day gives an average rate for B of $4.04'' \pm .34''$ during a rhythm following this it is $4.08'' \pm .14''$; for F, normal $4.04'' \pm .77''$ rhythm-curve $4.01'' \pm .096''$; for G, normal $3.47'' \pm .1''$ rhythm-curve $3.35'' \pm .17''$. It is evident, then, that whatever significance respiration may have on the conscious side, physically there is no ground for belief, on the basis of some fifty kymographic records of the rates of breathing during rhythmical perceptions, that there is actual physical correspondence between this rate and the rate of the rhythm perceived.

We tried the effect of attempting to rule out, by means of an instruction (*B 3 x*), the conscious presence of kinaesthesis during rhythmical perception.[65] These are our results:

(B) Uncertain whether there was relevant kinaesthesis or not. I felt before starting that I did not know what to do to inhibit kinaesthesis,—that if the instruction did not work automatically, I should be helpless. During fore-period verbal imagery (auditory-kinaesthetic) kept coming up: 'no kinaesthesis,' 'don't,' *etc.* Intense chest-strains and strains (vague) in arms and legs, all meaning resolve to inhibit. Auditory perception of first member was followed immediately by increase of chest-strains and general bodily tenseness, meaning: 'now,' 'don't get kinaesthesis.' . . . Perception of second member was at first purely auditory. Then it became a complex with other processes meaning: 'this is different'—from first member. Most prominent was visual imagery of a black and white streaked, almost formless thing, floating a little below and to the left of center of visual field meaning second member. The blackness of the image meant 'this is blacker than No. 1' thus carrying the distinction. . . . I should say the order of clearness was: clearest, first auditory sensation, then visual imagery; less clear, but quite clear, kinaesthesis relevant to effort to inhibit kinaesthesis relevant to grouping; unclear, kinaesthesis relevant to grouping, if present, and auditory sensations after visual sensations had appeared. . . . In general the effect of the *Aufgabe* was to make the experience very kinaesthetic in that whatever processes there may have been relevant to the grouping came upon a very complex background of fairly clear kinaesthesis. The experience was unusually kinaesthetic,—perhaps, less relevantly kinaesthetic. [*Rhythm: d', c'*].

[65] This instruction is similar to *B 2 g, q. v.*

(F) Lay back passively at first. But again, as soon as rhythm was 'going nicely,' as soon as I was 'in the swing of it' thoroughly and 'felt at home,' I noticed again the tendency of the throat to follow or sing the rhythm. Tried to stop this and did so, though I think it also involved holding breath. Now when throat was tense and I again put full attention on sounds, I found that my head tended to nod forward with accented beat. I simply couldn't prevent it. In résumé, I tried to attend passively, had actively to stop throat kinaesthesis. Then holding throat steady, I would no longer attend passively to tone but had to attend actively. When I began to attend actively, my head began to nod (actually, or in image), and I couldn't prevent it. [*Rhythm: d', c'*].

(G) With first two auditory sensations, strain sensations in head and unpleasantness, meaning: 'scrappy.' Then with the auditory sensations, vague taggy kinaesthesis in throat and slight strain-sensations in temples and head were the constituents of a disjointed talk to myself relevant to the tones. Then bobbed up for a moment a mass of unanalysed organic sensations in trunk meaning: 'I have to judge the rhythm clearly, two successive sounds as a group: no kinaesthesis.' Then just auditory sensations, tones, and a slight feeling of relaxation in trunk and pleasantness and beginning of vague kinaesthesis in front of chest, a sort of pressure inward. Then organic sensations meaning: 'that won't do.' Then very intense strain in top of head, forehead, temples, and also with each tone a pressure in ear, located near ear-drum, if not there. This becomes somewhat clearer. Organic sensations meant: 'that I could be positively certain that they were in groups of two members.' [*Rhythm: d', c'*].

We note that under the instruction consciousness becomes unusually full of strain sensations and other kinaesthetic complexes because of the effort to inhibit. We also note that in several cases, for moments in the periods of both B and F, a purely non-kinaesthetic rhythm was experienced. The rhythm, it is true, is under these conditions not permanent, nor is it actively perceived as a rhythm; nevertheless, a certain perception of rhythm is possible without kinaesthesis.

We next ventured into an investigation of light-rhythms, again by the method of fractionation (series *L d, e,* and *f*). All *O*'s perceived a rhythm. The three periods were in contents much what we have described in regard to other series, with the exception that kinaesthesis of eye-movements was very marked in all of the introspections—a result probably due to the extensiveness of the flashes on the screen. Although some of the kinaesthesis did carry the meaning of relaxation in the third period, especially for G, this was of far less frequent occurrence in this series than in any of the others. Otherwise the reports are not sufficiently different from reports of corresponding periods in other series to warrant insertion.

In answer to the 'confrontation-question' put to B, D, F, and G, the following statements were made:

(B) I think, although I am very doubtful, that the fore-period sometimes contained expectation carried in kinaesthesis. I am also inclined to believe that the rhythm was sometimes anticipated and I think that this anticipation was in terms of kinaesthesis. Probably there was visual imagery, too; again I do not remember. I am pretty sure that sometimes in the mid-period the rhythm was perceived before the second member occurred, the second member being carried anticipatorily in some manner,—sometimes, I believe, by organic chest-strains which immediately relaxed or changed as soon as the second member appeared. The change meant, I believe, 'the rhythm is perceived.' Kinaesthesis in the after-period carried impatience for the rhythm to conclude.

(D) As I now remember the experience there was (1) considerable kinaesthesis during the fore-period—in the region of the diaphragm (from rigidity of posture, and slightly inhibited breathing), and around the eyes (from the fixation of the region of the right ear). If the fore-period lasted, there was gradual relaxation with corresponding lowering of kinaesthesis. (2) The first sound of the rhythmical series was accompanied by heightened kinaesthesis, I believe (greater strain of bodily posture). There was a great deal of eye-movement kinaesthesis accompanying the auditory impressions with increased violence of 'diaphragmatic sensations' (constrained breathing) until 'clear perception of the group as a rhythm.' Kinaesthesis of nodding of the head usually accompanied auditory impressions. (3) This clear perception was accompanied or immediately followed by a reduction in amount, intensity, and kind of kinaesthesis. The sensations in diaphragm-region became those that mean easier, fuller breathing; they became less relevant to the situation, and on much lower level of clearness. There was gradual relaxation of muscles around eyes, with corresponding reduction in kinaesthesis. If there had been sensations from nodding of head or trunk in time to the beats, this was gradually much reduced. If the period continued for a long time, the kinaesthesis was ultimately much reduced. If now the period continued *ad nauseam*, there was a new set of kinaesthesis (though visceral sensations predominated). I should not swear that relevant kinaesthesis ever wholly lapsed.

(F) In the fore-period, always or nearly always, I think my introspections will show strain sensations of considerable intensity, of considerable extent (distributed through especially the chest, shoulders, face—especially in eye-muscles—and about the ears). It seems to me that in general these strain-sensations keep up (without any marked decrease in intensity—without alternation of strain and relaxation) until sound is heard. Before the time when the period was divided, certainly I should say that in the fore-period, these strain-sensations were not as intense. When the *Aufgabe* is to introspect from the beginning to the judgment (period 2) my general impression is that the strains in the fore-period were less intense than when the instruction was on period 1. Certainly the sounds are the processes which form the focus of attention; with the coming of the sounds there is a relaxation of the strain. When the sounds are heard, not in a rhythm, there are sometimes, I think, feelings of confusion which are partly kinaesthetic—not so much strain, I think, as muscular and articular—not so definitely localised, not so intense, and

not so clear as are the strain-sensations of the fore-period. The strains in the eye-muscles still persist after the coming of the sounds with little or no decrease in intensity, but certainly with decreased clearness. Let me add: that it seems as though the eye-strain and the ear-strain in the fore-period were fused with the other strain sensations—forming part of the general 'expectancy.' They persist at a lower level of clearness. What there is different about consciousness when the rhythm is perceived, is very difficult to say: there is, of course, a lack of 'confusion.' The sounds themselves are clearer than when they are heard not in rhythm. Sometimes there is kinaesthesis in the throat which begins when the perception of the rhythm comes, sometimes there is actual or imagined nodding of the head, but I am not sure but that this is true especially when the rhythm is first perceived and that both of these may be lacking and the rhythm be perceived. I am inclined to think that the kinaesthesis in the throat especially is a process which comes in partially or entirely because I have to judge pitch—and also I am not sure but that the nods of the head come in, partly because I formerly had to judge intensity and partly because nods of the head seem in a way to keep the two members of the group more or less apart so that the judgment of pitch is easier. My *Aufgabe,* expressly given by *E* or given to myself with regard to my behavior (conduct after the judgment, period 3), has been extremely varied—therefore not enough alike to generalise.

(G) During the judgment of pitch, intensity, and duration, only weak kinaesthesis was present with each auditory sensation, and sometimes, maybe, not at all (this last point very uncertain). During this time (time of judgment) muscular sensations meaning strain were present; sometimes weak, sometimes intense. After judgment, muscular sensations, meaning relaxation, gradually get more intense and clearer, and then remain at fairly constant intensity and clearness; and the kinaesthesis with each auditory sensation becomes more intense and clearer—gradually, and finally does not vary much. Usually the kinaesthesis is located on one side of the body with the first auditory impression, and on the other with the second. Kinaesthesis nearly always in trunk—though its exact localisation and extent at times (not very often) vary somewhat with or during various experiments. I remember, with some uncertainty, when it was localised in head for part of the experiment (when I did not have to introspect that period). When I had to judge and 'give myself up to the rhythm,' kinaesthesis (although varying in intensity and clearness) was, on the whole, more intense and clearer than when I was to judge in the first place.

When allowance is made for individual variations in these reports, our analysis of the typical consciousness does not lack confirmation. It is plain that kinaesthesis shifts both in formation and in meaning through the period.

IV. Conclusion and Summary

We have obtained a sufficient number of introspective analyses of the rhythmical consciousness from adequately trained observers, and under the isolating conditions of an

experimental procedure, to make possible what we consider valid conclusions. They are as follows:

1. There is a decided change in the kinaesthetic processes present in a rhythmical consciousness from the time that the first auditory impressions which form a rhythm are heard to the end of the period of the experiment. These processes vary (a) in texture, *i. e.*, there is a substitution of qualitatively different processes; (b) in clearness; (c) in intensity; and (d) in meaning or reference.

2. There may be a perception of rhythm without accompanying kinaesthesis, in terms of (a) visual imagery, or (b) auditory imagery, or sensation.

3. There is usually a marked change in the affective tone throughout a typical period of rhythmical perception, from slight unpleasantness before the rhythm is grasped, through pleasantness when it is thoroughly perceived, to unpleasantness when it continues without change.

4. There are individual variations in the amount of kinaesthesis, in the degree of its prominence, and in the type of meaning of kinaesthesis that corresponds to the rhythmical perception.

5. Generally, kinesthesis is most prominently connected with the initial clear perception of the type and form of the rhythm.

6. Instruction is almost invariably carried in consciousness by kinaesthesis.

We might sum up our main conclusions in a single formula: Under the conditions of these experiments, it proved that, whatever was the material presented for rhythmisation (equal and equally spaced sounds for subjective rhythm; sounds of different intensities; tones objectively varying only in duration, in intensity, in pitch; flashes of light differing in intensity), kinaesthesis was essential for the establishment of a rhythmical perception. That perception once established, however, rhythm might be consciously carried, in the absence of any sort of kinaesthesis, by auditory or visual processes.

LUTHER'S EARLY DEVELOPMENT IN THE LIGHT OF PSYCHO-ANALYSIS

By PRESERVED SMITH, Amherst, Mass.

"No villain need be; passions spin the plot;
Men are betrayed by what is false within."
—*George Meredith.*

Nowadays in all lines men are turning less than formerly to dramatic incident, and more to psychological struggle; less to the outward phenomenon and more to the inward cause. Novels and plays alike depend less than they once did on catastrophe and more on internal development. Historians, too, are laying less stress on plainly visible signs than on less obvious spiritual causes.

In the Reformation, for example, the Diets of Worms and Augsburg are not only exciting but they are significant moments, for they focussed the attention of Europe. Yet they would never have been possible but for long and silent preparation, an important part of which took place in the solitary cell of a Wittenberg cloister. Far be it from us to attribute the greater part of any strong historical movement to the influence of a single individuality. On the other hand let us not, in our desire to simplify, forget the importance of personality. Few great revolutions have been more dominated or more fully represented by a single man than was the Reformation; the age thought his thoughts and spoke his words, and, in short, received a durable impression from his genius. Realizing the tremendous importance of many other factors in the movement, let us for the moment devote our study to this one, asking what manner of man was Luther, and what were the sources of his spiritual power. Such questions are, of course, capable of only an imperfect answer. Psychology has hitherto hurled itself on the problem of genius as on an impassible barrier. It is quite certain that the whole of a man's life is accounted for by natural causes, the elements of his heredity and environment, but at present so large a portion of these forces are and must remain unknown, that we are forced to act on the maxim somewhere enunciated by William James: "The originality of a man does not date

from something anterior; on the contrary certain other things date from it." But while fully realizing that all cannot be explained, we believe that very much can be ascertained by careful study of the materials at hand and by the comparative method.

In undertaking a candid, and, as far as in me lies, a scientific examination of a great man's psychological origins and growth, I must protest, against the possible offence that some of my conclusions may give, that nothing is less my purpose than "to drag the radiant in the dust." It is surely evident that great men are subject to exactly the same natural laws as their fellows. The common assumption that spiritual value is undone if a lowly origin be asserted is surely false, as James[1] has so well pointed out; far more than we realize or like to admit, our highest impulses of love, religion, and morality are rooted in physical, even in pathological conditions. If the branches of the tree reach toward heaven, its roots strike deep into the dark bowels of the earth.

Approaching our subject then, exactly as we would any other man, and taking up first his heredity, it is difficult to deny that he inherited a taste for drink and possibly some of the defects that go with that diathesis. His father, like so many of his contemporaries, drank to excess, that is, to the point of intoxication.[2] Martin himself apparently avoided this extreme, but that he drank a great deal more than was good for him cannot honestly be denied.[3] It is possible to see a consequence of this in the weakness, both of mind and body, of his second son, also named Martin.[4] The direct results of alcoholism are as yet problematic; but it is generally agreed, and this is all that is claimed in the present study, that it probably conduces to mild forms of neurosis and unhealthy excitability. In Luther's case this showed itself in his most

[1] W. James. *Varieties of Religious Experience*, 1908, pp. 10ff.

[2] E. Kroker. *Luthers Tischreden in der Mathesischen Sammlung*, 1903, No. 193.

[3] On this, my *Life and Letters of Martin Luther*, 1911, pp. 318ff, and H. Grisar, *Luther*, ii, 1911, 244-65.

[4] In a play called "Maternity" (English translation in *Three Plays by Brieux*, 1911), the French physician Brieux has given a terrible picture of a case parallel to that which we are studying; an old peasant who occasionally became drunk has a son who stops short of this but drinks steadily and much, and as a consequence has feeble offspring. But this conclusion may be denied.

A German physician named Drenkhahn believes that as alcoholism decreases other nervous diseases increase. An answer to him by E. Bleuler, Alkohol und Neurosen. *Jahrbuch für psychoanalytische und psychopathologische Forschungen*, 1912, iii, 848ff.

conspicuous fault, a hot temper and almost unbridled violence of language.

His childhood was very unhappy. Almost all his reminiscences of it are either of corporal chastisement or of spiritual terrors. His father was a sturdy character, but hard; he once whipped his boy so severely that the latter hated him for it and fled from him for a season.[5] His mother, too, beat him at one time for stealing a nut, until the blood flowed;[6] but in her case he seems to have borne no lasting resentment, for, on recounting this incident many years later he immediately adds that such discipline was meant heartily well. It was with tender affection that he remembered his mother's pathetic little song:

> If no one's kind to you and me
> The fault, I think, of both must be.[7]

The sexual life of the child begins sooner than is realized by most adults, cut off from their infancy by a curtain of amnesia. This would not be worth while noting for itself alone; but the momentous results from these first impressions for the whole spiritual life of the individual, justify or rather necessitate a close examination of this otherwise unpleasant side of the child's experience. Now Luther is a thoroughly typical example of the neurotic, quasi-hysterical sequence of an infantile sex-complex; so much so, indeed, that Sigismund Freud[8] and his school could hardly have found a better example to illustrate the sounder part of their theory than him.

According to these psychologists, the first sexual feeling of the child is usually evoked, quite unconsciously of course, by one of the parents. The boy, in total ignorance of the nature of his own feelings, finds himself his father's rival for his mother's love.[9] This gives rise to jealousy and hatred of the

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[5] *Luthers Tischreden*, ed. Förstemann und Bindseil, 1844ff, iv. p. 76.

[6] Kroker, *op. cit.*, no. 753.

[7] *Luthers Werke*, Weimar, XXXVIII, 338.

[8] Freud's writings are full of this: *e.g.*, 'Eine Kindheitserinnerung des Leonardo da Vinci. *Schriften zur angewandten Seelenkunde*, VII, 1910. *Cf.* O. Rank, *Der Mythus von der Geburt des Helden*, 1909, with quotation from Freud, p. 7. E. Jones, *Der Alptraum in seiner Beziehung zu gewissen Formen des mittelalterlichen Aberglaubens*, 1912.

[9] "The first sexual excitation of the boy comes from the mother; his first hatred is for the father." Quotation from Freud, in O. Rank, *op. cit.*, p. 7.

father,—just as we have seen to have been the case with Luther,—and these feelings are soon transferred to other adult males. As the consciousness of sex emerges, whatever is connected with that is noted by the child and thus stamped upon his memory. As soon as Luther went to school he had the same unhappy experiences with his teachers that he had had with his father. He not only records with justifiable resentment that one morning he was whipped fifteen times without fault,[10] but also he remembered the obscene jokes made by his pedagogues so plainly that nearly fifty years later he was able to repeat one of them to his students.[11]

The fascination of sex for the child is not purely erotic, but is also due to the mystery with which the subject is surrounded. He broods long over its problems, as for example how babies are born. While still in the uncertain stage Martin was intrigued and impressed by hearing that a pretty young woman of the village had brought forth a dormouse.[12]

When first the secret of marriage is revealed to the boy, he naturally applies his knowledge to himself, particularly in that relation in which he is most interested. It is undeniable that Luther's thoughts turned in this direction. The story which he repeated oftenest in later life was one of incest between mother and son.[13] He says he heard the scandal while a student at Erfurt; but his mind must have been in some measure prepared for it, or it would not have made so deep an impression on him.

At this sensitive stage the boy's mind was filled with spiritual terrors. The devil intervened in all daily life; he and his angels made storms;[14] the child could not throw a stone in a lake without provoking the devils living in the water to raise a tempest.[15] There was a witch who plagued his mother, for she could make children cry themselves to death.[16] What anguish there must have been in this thought for the child! And this old woman, too, or at least one like her, was connected in his early thoughts with the mysteries of marriage.

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[10] G. Lösche, *Analecta Lutherana et Melanthoniana*, no. 545.
[11] Kroker, *op. cit.*, no. 753.
[12] *Commentary on Genesis*, *Werke*, Weimar, XLIII, 692.
[13] *Colloquia*, ed. Bindseil, I, 68; II, 367. *Tischreden*, ed. Förstemann und Bindseil, IV, 78. Lösche, *op. cit.*, no. 308 with three parallels in unpublished manuscripts of table talk. *Corpus Reformatorum*, XX, 589. Kroker, *op. cit.*, 706, 90. It also occurs elsewhere in his voluminous and poorly indexed works.
[14] Kroker, *op. cit.*, no. 170.
[15] *Lauterbachs Tagebuch*, ed. J. K. Seidemann, p. 65.
[16] *Tischreden*, ed. Förstemann und Bindseil, III, 96f.

A story he "heard as a boy," and related many years after,[17] was about such an one who showed herself in malice the superior of the devil; for when the evil spirit had tried in vain to separate a man and wife, she accomplished the result by hiding a knife under their pillow, thereby giving the husband the idea that his spouse intended to murder him, with the effect that he murdered her. This story, so banal in itself, stamped itself upon Martin's memory because it was the expression of a suppressed dread; the fear that the witch might do something even worse than make him cry himself to death, might separate his father and mother, turning the hated parent against the dear one. No one familiar with the researches of Freud and his scholars into the mind of the child will find this explanation of the story far-fetched or other than necessary. It is truly remarkable that of the very few recollections of his early childhood related by Luther in his table talk, so large a proportion should support the theory here advanced of the deep influence of this infantile complex on his subsequent nervous and spiritual development.

This influence showed itself in two ways; first in his obsession by the devil, secondly by the peculiar part played in his growth by the idea of concupiscence. The former is usually dismissed with a few words about Luther being a man of his age, emerging from the superstitious peasant class. But this only explains the smallest part of his belief, namely the form which it took. This, undoubtedly, he borrowed from the Bible and the current German demonology. But the nervous diathesis which made the devil so real to him, in short the obsession, the "Zwang," was peculiar to himself. Careful study into the lives of his contemporaries, Erasmus, More, Melanchthon, Calvin, show that although they inherited much the same superstitions as he did, believed in the devil and occasionally attributed great misfortunes, public or private, to his direct interposition, their personal consciousness was untroubled by what with Luther was the most real and terrible experience in his life. The fact of the obsession might be verified by hundreds of citations[18] from the Wittenberg professor's works and table talk. A glance at the relevant words in the indices of these volumes will turn up any quantity of descriptions of how the devil appeared at night to dispute with him, and of the vividness and horror of his apparition. The most graphic description of these visions is, perhaps, that in

[17] Preger, *Luthers Tischreden nach Schlaginhaufens Aufzeichnungen*. No. 196.

[18] A collection of them in Grisar, III, 231ff.

the book *On Private Masses and Ordination*,[19] in which a long argument with the Prince of evil is fully set forth. When the spirit appeared Luther tells how his heart beat, so that he almost died on the spot. But such visits were nothing extraordinary. He assures us he disputed with the devil every night.[20] Dreams came to him because Satan was there to prevent his resting, for "the devil can torture me so that sweat pours from me in sleep. . . . . My hardest battles have been in bed."[21] About the year 1513, while studying in the Wittenberg cloister at night, he heard the devil roar thrice from hell, and had not the courage to wait to hear more.[22]

Turning now for an explanation of this clearly abnormal condition to the specialist in nervous diseases, we read[23] that such obsessions are due to suppressed subconscious forces; devils, psychologically considered, are functional symbols of the repressed but not eliminated elementary sexual life. The origin of the belief in the nightmare, the incubus and the personal devil, is, in short, due to a condition of the nerves, frequently brought on by an abnormal infantile sexual life. Belief in such devils is but a projection of early experienced dread and forcibly repressed wishes, the wish namely to imitate certain functions of the father and the wish to spite him.[24] In a sense the image of the devil is but a projection of the image of the father. A commonly observed proof that these superstitious obsessions are really connected with the infant's sexual life, and one particularly prominent in Luther's case, is the prevalence of disgusting methods of putting the fiend to flight. These are neither more nor less than the child's ways of spiting its parents, and at the same time gratifying a primitive sex impulse.[25] If there is anything in this hypothesis, which seems to be established by broad observation, Luther's case offers strong independent support of it. He states over and over that he found argument of no avail

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[19] "Von der Winkelmesse und Pfaffenweihe," 1533, *Werke*, Weimar, XXXVIII, 197f.

[20] *Tischreden*, Weimar, I, 469. *Cf.* no. 802.

[21] *Ibid.*, 508. References might be multiplied, *e.g.*, *Tischreden*, ed. Förstemann und Bindseil, III, pp. 4-96, *passim*.

[22] *Tischreden*, Förstemann-Bindseil, III, 93. Freud relates a closely parallel case from his own experience, interpreting the hallucination as the child's remembrance of hearing its father snore. *Kleine Schriften*, II, 71.

[23] E. Jones, *Der Alptraum*, p. 70.

[24] *Ibid.*, p. 71.

[25] *Ibid.*, pp. 91, 95.

with Satan, but only some words[26] of untranslatable coarseness, or some act[27] or gesture which is simply unimaginable to persons with no first-hand knowledge of the Reformer's conversations. It is true that he inherited a certain body of these ideas along with his contemporaries Bugenhagen and Cellini;[28] nevertheless it is undeniable that they received from him an emphasis rarely if ever found elsewhere, and only explicable on the hypothesis of real neurosis.

Luther himself was, of course, unable to analyse his own feelings, but he showed surprising insight by the remark that it was the harsh discipline of the home that drove him into the monastery.[29] It was this cruel experience which gave him the dread of the powers of the other world, and which also emphasized the idea of concupiscence as the very instigation of the devil, to be fought against and crushed out at any cost.

A powerful impulse in the same direction was given by the meeting in 1497, when Martin was attending school at Magdeburg, with a Prince of Anhalt who had renounced his worldly position and become a Franciscan, in which quality "he so fasted, watched, and mortified his flesh that he looked like a death's head, mere skin and bones."[30] This was Brother Lewis,[31] born a Prince of Anhalt-Zerbst in 1456, and baptized William. He had taken the vows in 1473, studied theology, and engaged in deeds of charity as well as in works of self-mortification. In 1497,[32] when Luther saw him, he was at Magdeburg engaged in the pious office of mediating between the citizens of that city and their archbishop Ernest. He died in 1504 at Marburg. The extant fragments of his writings show that he emphasized the fear of God and the day of judgment,[33] though he did not forget to mention the love of Jesus. Luther speaks only of having seen the man, but it is

[26] "So kompt der teufl baldt und disputirt mit mir so lang bis ich sage: leck mich in dem a—." Preger: *Luthers Tischreden nach Schlaginhaufens Aufzeichnungen*, no. 31.

[27] If argument does not help, then, says Luther, "weyse man den Teuffel flugs mit eim furtz ab." *Tischreden*, Weimar, I, 469. *Cf.* p. 64. *Cf.* "Bugenhagen's way" of putting the devil to flight, Lösche, no. 337. *Cf. Tischreden*, Weimar, nos. 83, 812. Grisar, III, 255.

[28] On Bugenhagen, last note. For Benvenuto Cellini, his *Memoirs*, book I, chap. 64.

[29] Kroker, *op. cit.*, no. 753.

[30] *Werke*, Weimar, XXXVIII, 105. *Tischreden*, ed. Förstemann & Bindseil, III, 303.

[31] L. Lemmens: *Aus ungedruckten Franziskanerbriefe des XVI Jahrh.* 1911. pp. 8-22.

[32] *Ibid.*, p. 17.

[33] *Ibid.*, pp. 15f.

probable that he knew him quite well, as he did later the priests and monks of Eisenach. His subsequent relations with the family of Anhalt were warm, and can be traced as early as 1515, when he had the friendship of Margaret of Anhalt,[34] wife of Prince Ernest (who died 1516). The influence of such a man as Brother Lewis in turning the susceptible boy from a wordly calling to "religion," as the monastic life was then significantly called, must have been marked. At Eisenach, where Luther attended school for three years after leaving Magdeburg, the priestly influences were very strong. He knew well the priest of St. Mary's Church, John Brown, and the Franciscans of the local chapter.[35]

At the university of Erfurt the influences pushing him towards the monastery must have been still stronger. The town was large and flourishing;[36] the students led a turbulent[37] and fast life, so that Martin himself branded the institution as no better "dann ein hurhauss und bierhauss."[38] This, however, only put into stronger relief the devotion of the numerous bands of monks.[39]

The studies at the university were naturally medieval in their presentation of life. Even those students who professed devotion to the humanities, cultivated the neo-classics of the Renaissance rather than the genuine models of antiquity. For example, when Martin heard his later opponent Jerome Emser lecture on comedy in the summer of 1504, the text was not Plautus or Terence, but Reuchlin's *Sergius*, the hero of which was a rascally monk, whom, in the opinion of Emser, Luther

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[34] This little known fact is proved by Luther's letter to Margaret, November 4, 1519, published in *Mitteilungen des Vereins für anhaltische Geschichte und Altertumskunde*, 1904, x. 137f. In this he speaks of not having seen her for a long time, and that he knew her in or before 1515 may be inferred from the letter of his friend Link to Margaret of January 22, 1515. W. Reindell: *W. Linck aus Colditz*, p. 253.

[35] *Luthers Briefwechsel*, ed. E. L. Enders, I, 1, 3.

[36] A chronicler of 1572 called Erfurt the largest city in Germany. *Mitteilungen des Vereins für anhaltische Gesch.* x. 61. According to W. Köhler the town then had 32,000 inhabitants. *Im Morgenrot der Reformation*. Ed. Pflugk-Hartung, 1912, p. 347.

[37] *Cf. Eobani Hessi De Pugna Studentium Erphordiensium* . . . 1506, my *Luther*, p. 444. Sermon of 1539, Weimar, XLVII, 666: "At Erfurt I saw many killed."

[38] *Colloquia*, ed. Bindseil, III, 101. On the other hand I believe there is no authority for the story attributed to Luther about the assiduous attendance of the students on the lectures of Gambrinus and Tannhäuser, given, after Hausrath, by Dr. McGiffert: "Martin Luther," *Century Magazine*, LXXXI, 177.

[39] On this my *Luther*, 1911, p. 8, references, p. 442.

must have taken for his pattern.[40] The Reformer tells us that the first poet he read was Baptista Mantuan, then Ovid's *Heroides* and Virgil, after which the study of scholastic theology prevented the perusal of any more verse.[41] If the frank sensuality of the one Roman author, and the pathetic grandeur of the other, would take him out of the cramped world he knew, their modern imitator, whom he first read, would impress the reader deeply with his own extremely cloistral spirit. The *Eclogues,* first published at Mantua in 1498, enjoyed immense and immediate popularity, and were frequently reprinted, among other places at Erfurt in 1501,[42] just as Luther was matriculating. The first six eclogues treat of love, with what Balzac calls "la friande concupiscence des ecclésiastiques;" that is, it is described as the most alluring but wickedest thing in the world, a passion inspired by no god but Satan.[43] The description of women is a choice specimen of monastic invective against the dangerous sex. Luther, whose mind was evidently much preoccupied with this side of life, read and repeated the lines over so often that forty years later he was able to quote them word for word:

> Femineum servile genus, crudele, superbum. . . . .
> mobilis, inconstans, vaga, garrula, vana, bilinguis,
> imperiosa, minax, indignabunda, cruenta,[44]

and so on for many lines. The seventh eclogue is on entering the monastery, to which "Pollux" is moved by a vision; the eighth is on the "religion of the shepherds;" the ninth on the morals of the Roman Curia, and the tenth relates a controversy between observant and non-observant monks. It can be no mere coïncidence that in several points these writings foreshadowed experiences soon to come into Martin's own life. The seventh eclogue particularly worked upon his impressionable, morbid fancy, preparing him for the vision he was soon to have. All the influences to which he was subjected worked on him in the same sense; they made him look with terror upon the world as the primrose path of dalliance to hell, with profound distrust upon his own powers of resisting its temptations, and upon the monastery not so much with dread as the painful means of avoiding damnation, as with a sort of love as the harbour and refuge from a sea of troubles

[40] G. Kawerau: *Hieronymus Emser,* 1898, p. 10.
[41] *Tischreden,* Weimar, I, no. 256.
[42] Last reprinted by W. P. Mustard, Johns Hopkins University Press. 1911. For the editions, p. 35.
[43] *Eclogue* II, 112.
[44] *Ecologue* IV, 110ff. Luther's quotation, Kroker, *op. cit.,* No. 729.

where at last a man might find peace from the terrible war of the flesh against the spirit and security from the frightful dream of the wrath to come. If, many years afterwards, he said that impatience and despair made monks,[45] he was thinking of the despair that came to him immediately after his profession; in 1515 he could assert that despair made not a monk but a devil, and that no one was ever a good brother who had not taken the vows from love.[46]

Yet it is possible that he would never have entered the cloister had it not been for a special experience so marked and sudden that as early as 1519 one of his friends compared it to the conversion of the Apostle Paul.[47] His mind was prepared by the terror caused by an epidemic of the plague in the spring of 1505. Such visitations, particularly in times before science had come to protect and strengthen men, frequently cause demoralization. In this case many of the students fled to their homes and some entered the monastery.[48] Martin, too, returned home, to find two of his brothers dead or dying of the terrible disease.[49] The state of his mind can easily be imagined as he turned back to the University. On the journey, on July 2, at Stotterheim, near Erfurt, he was overtaken by a thunderstorm, a meteorological phenomenon which throughout life he regarded as directly due to supernatural agency. In a particularly vivid flash of lightning, followed by a crash of thunder, his overwrought imagination saw a heavenly vision, a divine warning to leave a temporal for a spiritual vocation, and in a paroxysm of terror he cried out: "Help, St. Anna, and I will be a monk."[50] Once before, when he had wounded himself, he had called on the Virgin for aid in like manner,[51] but had stopped short of taking the vow. Now that he had uttered it, he felt it to be binding, and though he regretted it, honorably discharged his promise by entering the Augustinian cloister two weeks later.

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[45] *Tischreden,* Weimar, I, No. 1034. *Colloquia,* ed. Bindseil, I, 124.
[46] J. Ficker: *Luthers Vorlesung über den Römerbrief,* II, 318.
[47] Crotus Rubeanus to Luther, Enders, II, 208.
[48] Eoban Hess's poem on the plague, reprinted in my *Luther,* p. 444.
[49] According to a sermon of 1544; Scheel: *Dokumente zu Luthers Entwicklung,* 1911, p. 19.
[50] This is told most fully in a saying of July 16, 1539, in *Lutheri Colloquia,* ed. Bindseil, III, 187. The same story, differing in details, is related by Justus Jonas in 1538. Scheel: *Dokumente,* p. 30.
[51] *Tischreden,* Weimar, no. 119, where Luther gives the date Tuesday after Easter. The editor conjectures this was in 1503.

Leaving aside much that is otherwise interesting in the history of the next dozen years, and confining our study exclusively to the Reformer's inward development, we find the key to understanding it in the psychological truth that a man's reasonings, opinions, philosophy, are fully as much the effect as the cause of his outward experiences and actions. The reasons he gives for his belief are in fact but the explanations of the experience which led him to that belief. William James asserts[52] that emotions follow and do not precede the bodily state; that we fear because we run away, reverence because we kneel, and love because we kiss. Argument is proverbially useless in changing a man's opinions on the deepest things in life; his attitude towards them is so rooted in his temperament and general culture that only by changing them can we alter his belief. So with Luther: the dogmas of the bondage of the will and of justification by faith only, the foundation-stones of early Protestantism, were attained not by logical deduction from Biblical or any other premises, but merely as an interpretation of his own subjective life.[53]

The most general and long continued of these experiences was concupiscence, the importance of which in Brother Martin's development has already been recognized,[54] though the psychological reason for this, namely that events in his infancy had given this side of life an abnormal, in his own interpretation supernatural, importance has not been seen. It is true that many men, perhaps all men who have any spiritual life, have at times felt the severity of the war of the members against the spirit, the great paradox of wanting to do that which one hates. The annals of monasticism are full of such men, who rolled in snow and lashed themselves with thorns to keep the body under. But it is doubtful whether anyone ever felt the conflict more keenly than did Luther, or gave it the importance in his system that he gave it. Through it all it is to his great credit that, there is

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[52] *Psychology* (1890), II, 449ff.

[53] Father H. Denifle notices that Luther's opinions were won from his experiences. *Luther und Lutherthum*, I², p. 447. Prof. F. A. Christie notes the same of some of Luther's ideas. *Harvard Theological Review*, 1912, April, p. 243.

[54] *E.g.* by Denifle, *op. cit.*, by W. Braun: *Die Bedeutung der Konkupiszenz in Luthers Leben und Lehre*, Berlin, 1908, and now by W. Köhler: *Im Morgenrot der Reformation*, ed. Pflugk-Harttung, 1912, p. 371.

good reason to believe, he never sinned with women.[55] To those who realize what the struggle cost him, his repeated assurances that his public battles with princes and false brethren were easier than his inward struggles with the flesh and the devil, seem perfectly natural.

His writings and sayings in later life are full of strong expressions about the horrors of monastic celibacy. For one saying that as a monk he did not feel much lust,[56] there are scores which state the direct opposite. "It is easier," he once exclaimed, "to bear chains and prison than desire; he to whom the gift of chastity has not been given will not get it by fasting and vigils. It happened to me, though I was not much harassed, that the more I buffeted myself the more I burned."[57] The monks, he tells us elsewhere, were tempted by pollutions almost every night, so that they dared not celebrate mass at daybreak.[58] In one place he calls celibacy a terrible torture,[59] and in another, "a sort of secret homicide."[60] So exclusive was his preoccupation with this temptation that he says before his break with the Church he thought there was no sin but lust.[61] Similar expressions might be multiplied indefinitely, from his book on *Monastic Vows*,[62] his *Answers to Duke George*,[63] his *Disputations*,[64] and his letters.[65] Turning from late reminiscences to the early writings of the Reformer, his notes on Lombard's Sentences (1510-11) we get the same impression. The longest note[66] is on concupiscence, which is described as "the disobedience of the flesh and sin;" another entry[67] expresses the opinion that the lust of the flesh compels

[55] This is proved by the absence of self reproach, and of positive evidence, and by his own testimony, as *Tischreden*, Weimar, I, no. 121: "I never looked at women even when they confessed. . . . At Erfurt I heard none, at Wittenberg three only."
[56] *Tischreden*, Weimar, I, no. 121.
[57] *Colloquia*, ed. Bindseil, II, 352.
[58] *Ibid.* II, 355.
[59] *Ibid.* II, 364.
[60] *Tischreden nach Schlaninhaufens Aufzeichnungen*, no. 348.
[61] *Tischreden*, Weimar, I, No. 126.
[62] Reprinted Weimar, vol. VIII. P. 583 he calls monasteries "lupinaria Satanae." P. 585 he says: "No celibate is without lust."
[63] Both reprinted Weimar, vol. XXXVIII.
[64] *E. g.* P. Drews: *Disputationen Luthers*, 1895, p. 579.
[65] *E.g.* his letter to Reissenbusch, March 27, 1525. Erlangen ed. LIII, 286, *cf.* Enders, v. 145
[66] *Werke*, Weimar, IX, 73ff.
[67] *Ibid*, p. 75.

the spirit to be impure. A third note[68] makes a fine distinction between the guilt of original sin, which may be abolished, and concupiscence, which is evidently conceived as original sin itself, a doctrine, indeed, to which the Wittenberger held all his life.[69] The hardest part of the battle came in the first years for which the fewest contemporary sources survive. Moreover in his public lectures and sermons he would naturally say less about this side of his private life than he felt. Nevertheless the courses he gave on the Bible for 1513-1516 occasionally refer to the matter. In a number of places he glances at it; in one note, apparently about 1514 or 1515, he says that he has learned, both from his own experience and from what others tell him, that even after a man has banished impure thoughts from his waking hours the enemy attacks him in sleep.[70] The lectures on Romans, too, emphasize the bitterness of the war between flesh and spirit.[71] The effect of all this on Luther's theology will be presently shown in detail. Here in general it must be remarked that he discovered that the more he strove against lust the less he accomplished; concupiscence was, he confessed, invincible.[72] With all his frantic efforts he could do nothing against it, nothing to merit salvation or God's favor. Therefore he was doomed necessarily to reprobation unless God of his free grace had mercy upon him. His will was impotent; his works could not justify him, but only God could help him; consequently salvation came from faith in him alone. This in broad lines is the essence of his development in the cloister, but, of course, it was not so simple. Many influences came in to modify his evolution, which must therefore be studied in greater detail.

The first three years in the monastery were a period of great depression. This was partly due to the losing battle with the flesh, for he regarded desire in itself as evil, and when he felt it at once concluded that all was up with his salvation.[73] Partly, too, it was purely physical depression, a pathological condition of the nerves due to their overwrought condition at the time of entry, and to the severe and gloomy discipline

[68] *Ibid*, p. 75.
[69] *Tischreden*, ed. Förstemann und Bindseil, II, 10. *Werke*, Weimar, IV. 626. *Römerbrief, Scholien*, 107, 162. Grisar, III, 3.
[70] *Werke*, Weimar, III. 423.
[71] J. Ficker: *Luthers Vorlesung über den Römerbrief*, 1908, *Glosse*, pp. 58, 68, and often elsewhere.
[72] *Römerbrief, Scholien*, p. 110.
[73] His own words, Scheel: *Dokumente*, no. 52.

of the rule. Such states are not uncommon even in otherwise normal natures; some physical cause, in this case fasting and the self-inflicted pains of asceticism, produces a state of anhedonia.[74] in which melancholia often takes the form of believing oneself damned. Looking back on his experience, Martin could speak of the cloister as hell, in which the monks were lost souls.[75] In 1518 he described his own sensations as "so infernal that no tongue can tell nor pen write them, nor anyone who has not experienced them believe them; so that, had the agony endured a half or even a tenth of an hour, the man who felt them would have utterly perished and his bones have been reduced to ashes. Then God appears horribly angry, and so does all creation. There is no refuge, no consolation, either within or without, but all things speak accusation. Then the man weeps: 'I was cast out from before thy face,' nor does he even dare to say: 'Lord, correct me not in thine anger.' In this moment, wonderful to say, the soul cannot believe that it will ever be redeemed, save that it feels the punishment is not yet complete."[76] Remembering Dante and other medieval visions of future punishment we must allow that this frightful fear of hell, so little sympathetic to our sceptical age, was in part a natural consequence of the religion which concentrated men's thoughts upon the world to come. In part, however, and perhaps chiefly, the obsession of dread was a direct result of the morbid condition of the young monk's nerves. Another form of the same phobia was the conception of God as a cruel enemy, the root of which idea must be found in the thinker's neurosis, just as was his obsession by the devil. Luther himself vaguely felt the similarity of his feeling towards both God and the devil at this time when he said: "When I looked for Christ it seemed to me as if I saw the devil."[77] His dread of his Creator, in fact, could not be so much the effect as the cause of his conception of him, for the simple reason that long after his dread had departed, his conception of God, as a cruel and capricious tyrant, "who seemed to delight in the tortures of the wretched and to be more deserving of hatred than of love,"[78] remained precisely

[74] James: *Varieties of Religious Experience*, p. 145.
[75] *Werke*, Weimar, XXXVIII, 148.
[76] *Werke*, Weimar, I, 557.
[77] *Werke*, Weimar, XLV, 86.
[78] See more fully the quotations from *The Bondage of the Will* (1525) in my *Luther*, p. 208.

the same as before. Luther himself naturally attributed all his dread to the teaching of the Romanists: "They made of Christ nothing but a stern, angry judge, before whom we must tremble, as he would thrust us into hell. So they painted him sitting on a rainbow, with his mother Mary and John the Baptist on either side as intercessors against his frightful wrath."[79] Protestant scholars[80] have been at much pains to justify sayings like this by researches in the tracts of the opening sixteenth century, but on the whole they have failed. If there were representations of God's wrath, there was also emphasis on his love.[81] The only certain fact to which Luther testifies is his own subjective feeling, and of that he has no doubt: "I did not love, I hated the just God punishing sinners, if not with silent blasphemy, at any rate with a great wail of indignation, saying that it was not enough that God should beat down poor sinners, eternally lost, with original sin and his law, but that even his gospel added woe to woe."[82]

Angstneurose, if we may borrow from the Germans a word not exactly translatable by either "phobia" or "melancholy" or "obsession," shows itself in other ways, chiefly by a pedantic scrupulosity.[83] Such was exactly Luther's case. "I was a pious monk," he says, "and held to my rule so strictly that I dare assert that if ever a monk got to heaven by monkery I should have done so."[84] At the time of his novitiate the instructor of the young monks was John Jenser von Paltz, an old martinet, and a particularly firm believer in the efficacy of works.[85] He put his charges through a course of discipline which in Martin's case was the very worst thing possible. His life was now bounded by a vicious circle; the more he was depressed the harder did he ply the works of asceticism, and consequently, the worse became his mental and

[79] *Werke,* Weimar, XLVI, 8.
[80] *E.g.* G. Kawerau: *Luther in katholischer Beleuchtung,* 1911, pp. 52ff.
[81] Grisar, I, 153ff.
[82] Scheel: *Dokumente,* p. 17.
[83] S. Freud: *Kleine Schriften zur Neurosenlehre.* Erste Folge, 2d ed. 1911, p. 63.
[84] *Werke,* Weimar, XXXVIII, 143. *Cf.* also his remarks about hurting his health and wounding his conscience while a monk by "good works." *Ibid.,* XLIII, 615.
[85] On him, T. Kolde: *Die deutsche Augustiner-Congregation und J. von Staupitz,* 1879, pp. 168, 174ff, and the article in the *Realencyclopädie.*

physical condition. He referred the new access of despair to some fault of his own, and confessed all sorts of imaginary sins. "My confessor once said to me," he relates,[86] "when I kept continually confessing foolish faults, 'You are a fool; God is not angry with you but you with him.'" Again:[87] "I often confessed to Staupitz, not about women, but about real difficulties, and he would say, 'I do not understand you.' So said they all." It is possible, indeed, that these sins were not purely imaginary, but only exaggerated. The psychologists are convinced that when a person persistently accuses himself of things he has evidently not done, it is because he has really done things he is ashamed to confess. Numerous instances have been discovered of young people who have confessed to murder, arson and all kinds of crimes of which they were innocent, because their conscience was tormented by their inability to break themselves of the habit of self-abuse, which, they had often been told, destroyed all moral stamina.[88] The habit is unfortunately so common that there would be nothing surprising in finding it in Luther; and only slightly exceptional in his case is the seriousness with which he took it. Indeed he seems to hint at this practice in the saying last quoted; he confessed, "not about women, but about *die rechten knotten,*" real difficulties, or scandals, or hindrances.[89] In his lectures on Romans (1515) he speaks of the "voluntaria et solitaria pollutio" with sufficient detail to excite suspicion.[90]

Luther's malady occasionally resulted in characteristic *crises des nerfs.* On one occasion, according to Dungersheim and Cochlaeus, the brother found him rolling on the floor like one possessed, crying: "It is not I."[91] He also speaks in later life of the intense discomfort it often caused him to look upon the cross, a fact correctly interpreted by Grisar as a nervous symptom.[92]

His earliest extant letter (April 22, 1507)[93] shows that he was greatly wrought up over the prospect of saying his first mass. When he actually came to celebrate that event, the thought that he was going to address God in person horrified

[86] *Tischreden,* Weimar, I, no. 122.
[87] *Tischreden,* Weimar, I, p. 240. *Cf.* Bindseil: *Colloquia,* II, 290.
[88] S. Freud: *Kleine Schriften zur Neurosenlehre,* I, 87ff.
[89] *Knote* defined *Rätselfrage, Anstoss, hindernde Grund,* in A. Götze: *Frühneuhochdeutsches Glossar,* 1912, s. v.
[90] Ficker: *Römerbrief, Scholien,* p. 26.
[91] Grisar, III, 598.
[92] *Ibid.,* 708.
[93] Enders, I, 1.

him and almost made him flee "like another Judas."[94] Indeed he stopped, and would not have continued but for the sharp admonitions of his prior.[95] A similar experience occurred some years later, on June 7, 1515, when he was walking in a procession carrying the host through the streets of Eisleben.[96] This last experience (if the dating is correct) is the more remarkable as Luther's other extant writings show that by this time he had won considerable self-possession, whereas all the indications point to the conclusion that the earlier years were spent in anguish. The horizon began to clear about the time of his first call to Wittenberg (October or November 1508).[97] The change was due in a measure to the affection and help given by the Vicar General, John von Staupitz, without whom the younger man, in his own opinion, would surely have gone to hell.[98] But it was not the theology of his older friend that helped him, for the similarity of their views later exhibited was due to what Staupitz learned from Luther.[99] Martin was helped most of all by the more cheerful surroundings and by the active work of study and teaching, in which his abounding energies were no longer turned inward, but found an adequate outlet. Not only were his powers thus given scope, but his attention was turned away from excessive concentration on the war with the flesh to higher cultural ends. The psychologists have a special name for this diversion of these impulses from sexual to professional or intellectual interests; they call it "sublimation."[100] The fact itself, that it is the worst thing in the world for a hypochondriac to spend all his time attending to his own temptations and that the best thing for him is to get into a field of absorbing work, this fact is so evident that it needs no learned terminology to recommend it. The point to notice here is that a considerable measure of relief came to Luther as a result of purely external causes; not

[94] Kroker, *op. cit.*, no. 750.
[95] Schlaginhaufen, *op. cit.*, no. 324.
[96] *Tischreden*, Weimar, no. 137.
[97] His letter to Braun, March 17, 1509, shows that he is calmer, thinking of God as "our God, who will sweetly govern us forever." Enders, I, 4ff.
[98] Enders, XIV, 189.
[99] Kolde: *Die deutsche Augustiner-Congregation*, 296. Luther himself says that Staupitz helped him by turning his attention from "Satanic illusions" to the work of public teaching, Weimar, XLIII, 667.
[100] S. Freud: *Kleine Schriften zur Neurosenlehre*, 2te Folge, 1909, p. 186. This "Sublimierung" is sometimes the direct result of suppression of sex (p. 181), but abstinence does not always tend to develope liberators and men of action (p. 190).

as the consequence of a change of his ideas; least of all does it indicate that he had at this time acquired the doctrine of justification by faith only, for that was still in the future.[101]

[101] As I have recently maintained that Luther's "conversion,"—*i.e.*, the special experience which brought him the message of justification by faith,—came at this time (my *Luther*, p. 15), I must justify my change of opinion. The principal reason convincing me that Luther had *thus early* acquired his fundamental doctrine, was the story which Paul Luther remembered hearing his father tell in 1544 and which he wrote down thirty-eight years later. (Köstlin-Kawerau, I, 749; my *Luther*, p. 19). According to this while at Rome (December, 1510) Luther began climbing the Scala Santa, but suddenly remembering the text "The just shall live by faith," desisted. In any case the reminiscence of a boy of eleven, not written down by him until he was forty-nine, must be unreliable, but now that the story has been discovered in one of Luther's own sermons in a very different form, Paul's version of it must be abandoned. In 1545 (*Zeitschrift für Kirchengeschichte*, XXXII, 607) the Reformer says that while at Rome he ascended the holy stairs with the purpose of getting the soul of an ancestor (his grandfather?) out of purgatory, but that when he arrived at the top he thought, "who knows whether this prayer avails?" This is certainly no proof that he then had the doctrine of justification by faith alone. Rereading of the sources has convinced me that he acquired it in 1515 or 1516. I shall devote another article to the study of Luther's development between 1508 and 1516.

THE FLUCTUATION OF LIMINAL VISUAL STIMULI OF POINT AREA

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I. Introduction

In a series of articles published 1906-08,[1] the writer reported the results of an experimental study of the phenomena usually attributed to fluctuation of attention. These phenomena, it was claimed, belong to three sense fields: visual sensation, auditory sensation, and cutaneous sensation. The problem was raised, it will be remembered, in 1888 by Nikolai Lange,[2] who gathered together the instances of intermittence of minimal sensations and found for them a common explanation in the conception of an instable or fluctuating attention. The recurrent changes in the limen of sensation producing the intermittence are, he contended, due to involuntary changes in the degree of attention given to the stimulus. Previous to the series of articles mentioned above, two other explanations had also been given: (a) Involuntary changes in the adjustment of the sense organ in case of vision and audition, primarily accommodation in case of vision (Münsterberg,[3]

[1] C. E. Ferree: An Experimental Examination of the Phenomena Usually Attributed to Fluctuation of Attention, *Amer. Jour. Psychol.*, XVII., 1906, 81-120; The Intermittence of Minimal Visual Sensations, *Amer. Jour. Psychol.*, XIX., 1908, 59-129; The Streaming Phenomenon, *Amer. Jour. Psychol.*, XIX., 1908, 484-503.

[2] N. Lange: Beiträge zur Theorie der sinnlichen Aufmerksamkeit und der activen Apperception, *Philosophische Studien*, IV, 1888, 389-422.

[3] Hugo Münsterberg: Schwankungen der Aufmerksamkeit, *Beiträge zur experimentellen Psychologie*, Freiburg, 1889, 69-124.

Heinrich,[4] and Heinrich and Chwistek);[5] and (b) an overflow of excitation from the circulatory and respiratory centers in the brain (Münsterberg,[6] Lehmann,[7] Slaughter,[8] Taylor,[9] etc.).[10]

In the series of articles mentioned above it was shown on the negative side that in case of vision at least, intermittence cannot be ascribed to any of the previously mentioned causes; and on the positive side that it is a phenomenon of the adaptation and recovery of the sense organ. Intermittence was denied in case of minimal cutaneous sensation,[11] and the

[4] W. Heinrich: Die Aufmerksamkeit und die Funktion der Sinnesorgane, *Zeitsch. f. Psychol.*, XI., 1896, 59-76; and Ueber die Intensitätsänderungen schwacher Geräusche, *ibid*, XLI., abt. 2, 1907, 57-59; Zur Erklärung der Intensitätsschwankungen eben merklicher optischer und akustischer Eindrücke, *Bulletin International de l'Academie des Sciences de Cracovie*, Nov., 1898, 363-382.

[5] W. Heinrich und L. Chwistek: Ueber das periodische Verschwinden kleiner Punkte, *Zeitsch. f. Psychol.*, XLI., Abt. 2, 1907, 59-74.

[6] *Loc. cit.*

[7] Alfred Lehmann. Ueber die Beziehung zwischen Athmung und Aufmerksamkeit, *Philosophische Studien*, IX., 1894, 66-95.

[8] J. W. Slaughter: The Fluctuations of the Attention in Some of their Psychological Relations, *Amer. Jour. Psychol.*, XII., 1901, 313-334.

[9] R. W. Taylor: The Effect of Certain Stimuli upon the Attention Wave, *Amer. Jour. Psychol.*, XII., 1901, 335-345.

[10] Münsterberg ascribed to this overflow, in case of respiration, an effect on the muscular control of the eye. During inspiration there was more accurate control of fixation and accommodation and during expiration a less accurate control of these adjustments. Lehmann leaves us in some doubt as to just how he believes the effect is produced. He says (op. cit., p. 84): "Wir sahen dass die Reactionen am haufigsten sind in der Nähe des Inspirationsmaximums. Hier ist eben der Blutdruck am grössten, und von diesem Zustand muss angenommen werden, dass er für die psychologische Arbeit des Gehirns günstig sei. Wir wissen ja, dass das Blut, während der Arbeit irgend eines Organes, demselben reichlicher zufliesst. Deshalb ist es höchst wahrscheinlich, dass auch die Arbeit eines Organes erleichtert werde wenn durch irgend eine Ursache eine Vergrösserung des Blutzuflusses herbeigeführt wird." Slaughter and Taylor are inclined to believe that the overflow affects the sensory cells directly. In their experiments a plethysmographic record of the peripheral blood pressure was taken while the fluctuations of the visual stimulus were being observed. They conclude that their results show a coincidence between the maxima of the plethysmographic curve and the phase of visibility of the fluctuation record. Two kinds of maxima are found in the plethysmographic tracing, one due to inspiration and the other forming the crest of a long vaso-motor wave of unknown cause, commonly called the Traube-Hering wave.

[11] In 1907 the experiments in cutaneous sensation were repeated by Geissler (L. R. Geissler: Fluctuation of Attention to Cutaneous Stimuli, *Amer. Jour. Psychol.*, XVIII., 1907, 318-321). A mistake

phenomenon was left open for further consideration in case of auditory sensation. A part of the work done at this time still remains unpublished. Some of it covers points still in dispute. For that reason two articles will be added to the former series. The first is in answer to an article by Heinrich and Chwistek entitled: "Ueber das periodische Verschwinden kleiner Punkte,"[12] and is intended to clear up, if possible, at least so far as the writer's work is concerned, the last point in dispute between the adaptation and accommodation theories. Heinrich and Chwistek maintain that the fluctuation of minimal visual stimuli of point area is caused by periodic changes in the curvature of the crystalline lens and offer their results for stimuli of point area as evidence that the fluctuations of stimuli of all areas are caused by changes in accommodation. In one of the former studies[13] the present writer had worked with stimuli ranging from 2 mm. x 2 mm.—42 cm. x 38 cm. in area. He found that stimuli of these areas fluctuate just as readily for aphakial as for normal subjects, and that changes in accommodation, therefore, can not be considered an essential factor in the production of the phenomenon. It had never occurred to him to work with stimuli of point area. In the present study, however, stimuli

was made by him in interpreting the writer's method of stimulating the tongue electro-cutaneously that has not yet been corrected. He says, "In repeating Ferree's experiment with electro-cutaneous stimulation of the tongue, we found some difficulty in eliminating the touch, pressure, and taste sensations set up by the electrodes. The best results were obtained by applying a 1% solution of cocaine to the fore part of the tongue, upon which two strips of tin foil (Christmas tree foil), hammered as thin as possible, were laid. The strips were connected with the interruptor of a Du Bois-Reymond induction coil." Christmas tree foil was not used in the original experiments. This material was rejected at once by the present writer as unsuitable. It is much too stiff and gives rise to pressure sensations. Narrow strips of very thin and pliable wrapping foil were used instead. When these were placed on the fore part of the tongue moistened with spittle, the observer was utterly unable to tell whether or not they were in contact with the tongue when the coil was not working. Neither did they under the action of the current give rise to taste sensations. Of the two procedures the writer would prefer the one used in the original experiments. It seems to him obviously better to make the electrodes of wrapping foil than to use the stiffer material and cocainize the tongue into insensibility to contact, more especially since Geissler's observers report that the cocaine itself sets up distracting sensations in the tongue.

[12] W. Heinrich and L. Chwistek: *Zeitschr. f. Psychol.*, XLI., 1907, 39-74.

[13] See An Experimental Examination of the Phenomena Usually Attributed to Fluctuation, 98-108.

of point area have been used. From the results of this study it will be shown that the fluctuations of these stimuli present no especial case; for (a) they occur just as readily for aphakial subjects as for subjects with normal eyes; and (b) identified by the tests used by the writer in his earlier experiments they correspond just as closely to adaptation phenomena as do the fluctuations of stimuli of larger area. In the second paper, work on the fluctuation of auditory stimuli will be reported. In this work the writer has succeeded in getting conditions under which no fluctuations occur, whether the stimulus be tone or noise. His results also enable him to explain without recourse to central factors or the tensor mechanism of the middle ear the fluctuations which do occur under experimental conditions different from those he has used. The completion of these two pieces of work rounds up, so far as the writer knows, all of the outstanding points in his case against fluctuation of attention in its original meaning.

II. The Accommodation Theory

That involuntary changes in accommodation are a factor in causing the fluctuation of minimal visual stimuli was proposed first by Münsterberg in 1889.[14] Münsterberg held that the fluctuation of these stimuli is due to two causes: unsteadiness of fixation and involuntary changes in accommodation. Although different views may be held with regard to the essential physiological and psychological factors in attention, all must agree, he says, that when a visual stimulus is attentively observed the eye is fixated and accommodated so as best to receive the impression on the retina. But this adjustment cannot be uniformly maintained for any length of time. Involuntary changes both in fixation and accommodation occur. These changes weaken and confuse the light impression received on the retina, hence an object just noticeably different from its background will alternately disappear into this background and become distinct from it.

The effect of lapses in accommodation is too obvious, he thinks, to need special explanation. The rays of light are no longer sharply focused on the retina and the image of the object blurs and becomes indistinguishable from its background. For unsteadiness of fixation, however, the case is not quite so clear. The explanation is as follows. Fick, Kirschmann, and others have shown that the sensitivity of the retina to colorless light attains its maximum at a certain

[14] Hugo Münsterberg: Schwankungen der Aufmerksamkeit, *Beiträge zur experimentellen Psychologie, Freiburg,* 1889, pp. 69-124.

distance from the fovea. Thus when the eye loses its fixation, the image of the object fixated travels towards a more sensitive part of the retina. This will cause the image of the rings on the Masson disc, for instance, which in the traditional fluctuation experiment are made just noticeably darker than their background, to lighten and become equal in brightness to the background. This gives the phase of invisibility. When fixation is regained, however, the ring again becomes noticeable. This gives the phase of visibility. These two factors, then, the lightening of the image of the ring due to involuntary changes of fixation and the blurring of its outlines due to involuntary changes in accommodation, should, according to Münsterberg, be considered as the cause of the alternate appearance and disappearance of the rings on the Masson disc which were attributed by Lange to fluctuation of attention. Since the writer has already shown in his first article[15] that involuntary changes in accommodation cannot be considered as an essential factor in these fluctuations,[16] space will be taken here only to point out that changes in fixation should also not be considered essential factors in the sense in which Münsterberg considers them factors. In the first place they could, in any event, have an effect only in case the stimulus was darker than the background. If the stimulus were lighter than the background, the brightening of the image would make it stand out more distinctly from the background than before, instead of causing it to disappear into the background as it is observed to do in fluctuating. Moreover, the explanation can have little or no application to the fluctuation of colored stimuli. Since both of the latter classes of stimuli fluctuate just as readily as the former, the principle can be regarded as having little value for purposes of explanation. And in the second place, this factor could not in all probability even cause the fluctuation of stimuli darker than the background, for the increased sensitivity of the extra-foveal retina would not only cause the ring to brighten, but also the background immediately surrounding it. The effect of the factor would

[15] See An Experimental Examination of the Phenomena Usually Attributed to Fluctuation of Attention, pp. 84 and 94-96.

[16] In the original article stimuli ranging in area from 2 mm. x 2 mm.-38 cm. x 42 cm. were used. For the writer's observers fluctuations never occurred when a stimulus 38 cm. × 42 cm. or larger was observed at a distance of 1 meter. In the experimental portion of the present paper it will be shown that changes in accommodation are not an essential factor in the fluctuation of stimuli of smaller area than 2 mm. x 2 mm., namely stimuli of point area. Thus with the present paper the demonstration will have been finished for the whole range of areas for which fluctuation occurs.

thus be merely to raise both the gray of the stimulus and of the background in the brightness scale, not to make them equal, unless indeed one were affected more than the other by an amount that would be noticeable in sensation, which can hardly be possible since the difference between them is, to begin with, only just noticeable.[17]

Münsterberg supported his explanation by the following experimental evidence. The norm of the period of fluctuation was established for each of his subjects and the following variations of conditions were made. (1) A "prismatische Lorgnette" which moved the field of vision slightly to one side was placed before the eyes. When this was held steadily in position, the period of fluctuation was affected very little, but when it was removed and interposed every 2 seconds, causing the eye to move quickly to the side to follow the shift in the object fixated, the period was very noticeably lengthened. (2) Involuntary blinking was caused every 2 seconds by means of a sharp sound. Fluctuation was prevented. When the eyes were closed voluntarily every 2 seconds, the same results were obtained. This, Münsterberg thinks, was because of the relief of muscular strain produced by the blinking. That is, as the lids are closed, the eyes move downwards and inwards; as they are opened, upwards and outwards (Bell's phenomenon). This frequent relief from the strain of fixating and accommodating so freshens the muscles, he thinks, and improves their action that disappearance never ensues. (3) The whole apparatus bearing the Masson disc was slowly moved back and forth, up and down, and sidewise. Each movement was executed in 2 seconds. Thus, in order to fixate the moving stimulus, the eye was kept continuously moving. The accommodation was also kept continuously changing. In a companion series of experiments the head was moved slowly from side to side. In this case also, in order continuously to fixate the stimulus the eye was compelled to move.[18] Fluctuation did not occur in either series of experiments. Again Münsterburg thinks fluctuation was prevented because the muscles were kept in such a fresh condition that accurate fixation and accommodation could be maintained throughout the observations. A moment's reflection will show (1) that these assumptions cannot be wholly true. The attempt of the eye to follow the moving stimulus, whether the movement was apparent, produced by the interruption of the "prismatische Lorgnette" or actual, produced by the moving of the apparatus bearing the stimulus, must have resulted in the image falling now to this side, now to that side of the fovea. If so, the fixation maintained was far from accurate.[19]. Likewise when fixation was lost in blinking, it was doubt-

[17] If, for example, one were very much lighter or darker than the other, the greater sensitivity of the extra-foveal retina might affect one more than the other enough to cause a noticeable change in the difference between them, but this can scarcely be assumed to be the case when one is only just noticeably different from the other.

[18] The movement was equal in amount to the movement of the head and in the opposite direction.

[19] This frequent shifting of the image from the position previously occupied by it on the retina would give abundant chance for the adapting retina to recover, and thus in terms of the adaptation theory to explain the absence of fluctuation.

less regained through a series of small oscillatory movements as commonly happens before the eye comes to rest in taking a new position. And (2) even were the assumptions true, the argument is not at all differential for Münsterberg's theory. The same effect on fluctuation would be expected in terms of the adaptation theory. An abundant reason was given for the stimulus never disappearing in the effect of the eye-movement on restoring the adapting retina. Eye-movement, it will be remembered, exerts its effect on adaptation in two ways. There is (a) an indirect effect. As the result of the movement the image falls on a fresh area of the retina and the area previously stimulated is given a chance to recover. (b) There is a direct effect which is much greater than the indirect effect, namely, the influence of eye-movement upon the amount and direction of the lymph streams that are continually moving hither and thither in the retina. A detailed discussion of this effect was given in the writer's earlier work.[20] Eye-movement is thus an essential factor in both theories. The relation to fluctuation ascribed to it in the two theories, however, is very different. In Münsterberg's theory, eye-movement helps to cause the disappearance of the stimulus, while in the adaptation theory it is the most important cause of the reappearance of the stimulus. With regard to the relative merits of these two views, the writer will say that a few minutes' observation of a liminal stimulus should be enough to convince anyone that a voluntary eye-movement, for example, instead of causing the stimulus to disappear, will on the contrary serve to keep it distinct; and, if it has disappeared, will cause it to reappear. For a detailed demonstration that involuntary eye-movement acts in the same way and that it is the chief factor in rendering adaptation intermittent, see the writer's earlier articles "An Experimental Examination of the Phenomena Usually Attributed to Fluctuation of Attention," and "The Intermittence of Minimal Visual Sensations."

Münsterberg also conducted a series of experiments in which a comparison was made of the rate of respiration and fluctuation. The results showed that when the respiration was in short quick gasps, the rate of fluctuation was increased; and when it was slow, the rate of fluctuation was decreased. In explaining this result he attributes to breathing an influence on the muscular control of the eye. With the inspiration there is an increase of the muscular control; and with the expiration, a decrease.

The accommodation factor was next taken up by Pace.[21] Pace compared the fluctuations obtained by his subjects before and after the paralysis of their ciliary muscles by a solution

[20] See The Intermittence of Minimal Visual Sensations, *Amer. Jour. Psychol.*, XIX., 1908, 112-129; and The Streaming Phenomenon, *Amer. Jour. Psychol.*, XIX., 1908, 484-503.

In the blinking experiment ,in addition to the effect of the accompanying eye-movement, the blinking would have itself produced an effect on fluctuation. That is, the closing of the lid shut off the light coming from the stimulus and gave the adapting retina a chance to recover.

[21] Edward Pace: Zur Frage der Schwankungen der Aufmerksamkeit nach Versuchen mit der Masson's Scheibe, *Philosophische Studien*, VIII., 1893, 388-403.

of sulphate of atropine, and found no significant difference in his results. He concluded, therefore, that changes in accommodation could not be considered as essential to the phenomenon.

The theory, however, would not down. It was revived by Heinrich, and Heinrich and Chwistek in a series of articles extending from 1896-1907.[22] In the article entitled: "Die Aufmerksamkeit und die Funktion der Sinnesorgane," Heinrich establishes the following principles which he considers of importance in explaining fluctuation. (1) When the attention is directed away from optical impressions (a) the lens takes the curvature characteristic of far seeing, and (b) the lines of sight tend towards the parallel position. The demonstration of these principles, however, cannot be considered as having any very direct bearing on the explanation of the phenomenon of fluctuation, for it may very well be conceived that the voluntary direction of attention away from visual impressions would cause changes in the accommodation and fixation of the eye of a magnitude that would be significant, while the involuntary lapses of attention occurring during the prolonged observation of a stimulus would not cause these changes at all. At least for the purpose of explanation of the phenomenon of fluctuation, the demonstration of the former cannot be considered the equivalent of the demonstration of the latter. And (2) when the attention is directed away from all optical impressions, involuntary changes take place in the curvature of the lens. This conclusion is based upon the recurrent changes that take place in the breadth of the pupil and upon the behaviour of images reflected from the anterior surface of the lens. In drawing this conclusion from the first point of evidence, Heinrich obviously assumes a closer connection between the changes in the breadth of the pupil and changes in accommodation than can safely be done. Any one who has studied the reactions of the pupil under a very wide range of conditions can not help but know that this 1:1 correlation does not exist. Moreover, the connection has not been found in a large enough percentage of cases to make it safe, even plausible, to assume that it exists in any situation in which it has not yet been demonstrated. In his second point of evidence, Heinrich does not describe the behaviour

[22] W. Heinrich: Die Aufmerksamkeit und die Funktion der Sinnesorgane, *Zeitschr. f. Psychol.*, XI., 1896, 410-431; Ueber das periodische Verschwinden kleiner Punkte, *ibid*, XLI., 1907, 59-74, und Zur Erklärung der Intensitätsschwankungen eben merklicher optischer und akustischer Eindrücke, *Bulletin International de l'Academie des Sciences de Cracovie*, Nov., 1898, 363-382.

of the images observed. Apparently, however, the description is supplied in a later paper published in coöperation with Chwistek entitled "Ueber das periodische Verschwinden kleiner Punkte." At least a method of demonstrating changes in the curvature of the lens based on the behavior of the image reflected from its anterior surface is described here. But the validity of this demonstration is strongly open to question. In the experimental section of the present paper it will be shown that it is much more plausible to ascribe this behaviour to involuntary eye-movement than to involuntary changes in accommodation,—that in fact the same kind of behaviour has been described by de Schweinitz[23] and others, in case of the images reflected from the cornea, as one of the common phenomena of ophthalmometry due to eye-movement.

In the article entitled "Zur Erklärung der Intensitätsschwankungen eben merklicher optischer und akustischer Eindrücke,"[24] Heinrich discusses the effect of variation of intensity, or differences in intensity between the stimulus and its background on the fluctuation of visual stimuli. Marbe[25] had found that an increase of intensity increases the phase of visibility, and conversely a decrease of intensity decreases the phase of visibility.[26]

This, Heinrich thinks, is just what should be expected were the disappearances caused by recurring lapses in the adjustment of the lens. As will be shown in the next section of the present paper, however, these results offer no differential evidence in favor of the accommodation theory. They are just what would be expected in terms of any theory that has yet been advanced to explain the fluctuation of minimal visual stimuli. Heinrich also notes that for one of Marbe's observers the

[23] G. E. de Schweinitz: *Diseases of the Eye,* Philadelphia and London, 1902, p. 739.

[24] *Op. cit.,* 366-369.

[25] Karl Marbe: Die Schwankungen der Gesichtsempfindungen, *Philosophische Studien,* VIII., 1893, 615-637.

[26] Marbe apparently was the first to make any separation of the phase of visibility from the phase of invisibility in drawing his conclusions, and even he did not take any account of the phase of invisibility in making his comparisons. The total times of visibility of his stimuli under the different conditions alone were compared. This tendency to break up the total period of fluctuation into its phases for purposes of comparison was a step in the right direction, but it was little heeded by his successors.

Marbe concludes that neither the fluctuation of the "Schrödersche Treppenfigur" nor the visual sensation is periodic. The phase of visibility of the visual stimulus increases with the increase in the difference in intensity between the stimulus and its background. The length of the period of fluctuation is a function of this increase.

phase of visibility was decreased when the image of the stimulus fell on the paraxial portions of the retina. This also, he says, is just what should be expected were the disappearance caused by changes in the curvature of the lens. So is it also what should be expected were adaptation the cause of the fluctuation, for one of the most conspicuous differences between the phenomena in the central and peripheral retina is the greater rapidity of adaptation in the peripheral retina.[27] Again, then, the evidence cannot be considered as differential. Heinrich also takes into consideration in this paper the results of Pace with the atropinized eye. He claims that changes in the curvature of the lens may still be observed when atropine has been used to paralyze the muscles of accommodation. While the present writer by no means contends that the muscles of accommodation are completely paralyzed by the use of atropine, still he would maintain that Heinrich's claim is strongly open to question if it is based on the kind of observation described by him and Chwistek in the article "Ueber das periodische Verschwinden kleiner Punkte." In any event the point is no longer of importance to the explanation of fluctuation, for it has been shown since that time by the present writer[28] that eyes from which the lenses have been removed and which under careful test shown no residual accommodation, get the fluctuation apparently just as readily as the normal eye.

In the article "Ueber das periodische Verschwinden kleiner Punkte," Heinrich and Chwistek, using stimuli of point area, attempt as their *experimentum crucis* to demonstrate the

[27] In fact in the writer's own experiments, designed to show the correspondence between adaptation and fluctuation in the peripheral retina, the farther the stimulus was moved towards the periphery of the retina the shorter became the phases of visibility and the longer the phases of invisibility. These experiments were made differential for the adaptation theory (a) by the method of variation of areas, and (b) by showing a rough correspondence between the phase of visibility in the fluctuation experiments with the adaptation time for different visual stimuli from center to periphery of retina. No. 27 gray and the red, green, blue, and yellow of the Hering series of papers were used as stimuli. Only a partial list of the results obtained was published, however, because the writer did not at that time consider the phenomenon in indirect vision worthy of more space. The main arguments were established for direct vision and no more data were included for indirect vision that were needed to show in a general way that the phenomena here present no exception. (See "An Experimental Examination of the Phenomena Usually Attributed to Fluctuation of Attention," 116-119.)

[28] See An Experimental Examination of the Phenomena Usually Attributed to Fluctuation of Attention, 84 and 94-96.

coincidence of fluctuation and changes in accommodation. Heinrich had mentioned the desirability of making this demonstration in the preceding paper, but had discarded the idea as infeasible because of the conditions under which the experiment would have to be conducted. In choosing to work with point stimuli in this study, Heinrich and Chwistek have admittedly selected the conditions most favorable to the accommodation theory, for by Heinrich's own statement in the explanation of his theory in an earlier paper[29] changes in accommodation would be more apt to cause the disappearance of point stimuli than of stimuli of larger area. Notwithstanding this admission, however, the results they get with point stimuli are advanced in the later paper as evidence that the fluctuation of stimuli of all areas is due to involuntary changes in accommodation. Their work with stimuli of point area will be taken up in detail in the next section of this paper.

III. The Fluctuation of Stimuli of Point Area (the Work of Heinrich and Chwistek)[30]

Heinrich and Chwistek maintain that the fluctuation of visual stimuli of point area is caused by periodic changes in the curvature of the crystalline lens. They also offer their results for stimuli of point area as evidence that the fluctuations of stimuli of all areas are caused by changes in accommodation. Four arguments are advanced by them in support of this conclusion. (1) Periodic changes in the curvature of the lens are directly observable. Moreover, these changes are found roughly to coincide with the fluctuations of the point stimulus when both observations are conducted at the same time. They describe two methods of demonstrating this change of curvature. One may be considered popular, the other technical. The popular demonstration may be conducted as follows. Prick two holes in a cardboard nearer together than the breadth of the pupil of the eye. Hold the card close to the eye and look through the holes at a bright light. The holes will be seen as two dispersion circles with a bright overlapping area. When the curvature of the lens changes, the overlapping area alternately contracts and expands. That is, as the lens becomes more convex, the dispersion circles become smaller and the overlapping area becomes narrower; and conversely, as the lens becomes less convex the circles become

[29] See Zur Erklärung der Intensitätsschwankungen eben merklicher optischer und akustischer Eindrücke, 366-67.

[30] W. Heinrich and F. Chwistek: *Zeitschr. f. Psychol.*, XLI., Abt. 2, 1907, 59-73.

larger and the overlapping area becomes broader. This change in the overlapping area, they say, can readily be observed. Their technical demonstration was accomplished by means of an ophthalmometer. Their method was as follows. Two spots of light were thrown on the eye of the observer by means of two mirrors reflecting the light from a lamp properly placed with reference to these mirrors and to the eye of the observer. These images were observed by means of an ophthalmometer. Their description of method is extremely meager. They say: "Lässt man die beobachtete Person den Punkt fixieren, dessen periodisches Verschwinden untersucht wird, und dreht die Glasplatten des Ophthalmometers, bis man in dem Instrument die beiden von der vorderen Linsenfläche reflektierten Bildchen als drei Punkte sieht, so offenbart sich jede Krümmungsänderung der Linse dadurch, dass der mittlere Punkt bei grösseren Änderungen sich spaltet, bei kleiner breiter wird. Man beobachtet dann ohne weiteres, dass die Linseneinstellung nicht stabil ist, sondern dass sie kleinen periodischen Aenderungen unterliegt. Diese Aenderung konnte mit unserem Instrument durch die Drehung der Platten um höchstens 0.5° kompensiert werden. Es war uns unmöglich die Aenderungsrichtung aus den Bewegungen des Punktes zu erkennen."[81] While these changes in the image reflected from the observer's eye were being recorded by a second person, the observer himself recorded the fluctuations of a point stimulus. Simultaneous records were thus obtained which could be compared in order to determine whether the phases of fluctuations coincided with the phases of changes in the image reflected from the eye. The point stimulus consisted of a small black point on a white ground or a small white point on a black ground, 0.1-0.3 mm. in diameter, observed at a distance of 70-150 cm. Two observers, Herr Sk. and Herr Zacz, were used. The eyes of both were normal, or emmetropic. Chwistek recorded the changes in the images reflected from the eye. Their results are stated as follows. For observer Sk., 776 phases were recorded. "Einseitige Notierung vom Herrn Sk., d. h. notiertes Verschwinden des Punktes ohne entsprechend notierte Akkommodationsschwankung ergab sich in 38 Fällen. Einseitige Notierung vom Herrn Chwistek, d. h. notierte Akkommodationsänderung ohne entsprechende Aufzeichnung des Verschwindens des Punktes fand man in 40 Fällen." For observer Zacz, 296 phases were recorded. "Einseitige Notierung vom Herrn Zacz in 31 Fällen. Einseitige Notierung vom Herrn Chwistek in 32 Fällen."

[81] *Op cit.*, 60-61.

(2) Within the range of areas used by them an increase in the area of the stimulus was found to give longer phases of visibility and shorter phases of invisibility. And, conversely, a decrease in the area of the stimulus was found to give shorter phases of visibility and longer phases of invisibility. Points were observed ranging for one observer (emmetropic) from .2 mm.-.5 mm. in diameter, at distances ranging from 100 cm.-126.5 cm.; for another observer (2.5 myopic), .2 mm.-.5 mm. in diameter at distances ranging from 35 cm.-39 cm.; and for a third observer (4 D. myopic), .2-1.5 mm. at distances ranging from 15 cm.-28.5 cm.

(3) The phase of visibility was also found to vary with the intensity of the stimulus or with the brightness difference between the point and its surrounding field. The greater was this brightness difference, the longer the phase of visibility was found to be as compared with the phase of invisibility, and the less was this brightness difference, the shorter was the phase of invisibility.

(4) When the stimulus was placed just beyond the far point for an observer with myopic eye, it was found to become periodically more and less distinct. Also two points placed at this distance were found alternately to blur into one and to separate into two. Two observers were used in these experiments.

Before passing to his own experimental evidence that involuntary changes of accommodation are not an essential factor in the fluctuation of stimuli of point area, the writer has the following comments to make on the work of Heinrich and Chwistek. (1) In this work they have created for themselves a special problem, that is, they employed stimuli of point area and strongly supraliminal intensity. The fluctuation of such stimuli has never been ascribed to the fluctuation of attention. Historically considered, then, they are not working with the phenomenon to which they primarily make their conclusions apply; and, moreover, they have not in any way shown in a satisfactory manner the propriety of applying their conclusions to the phenomenon explained by Lange as due to the instability of attention. (2) Their popular demonstration of involuntary changes in the adjustment of the lens is strongly open to question. Employing 124 subjects, the writer has not been able to make it work in a single case in which care was taken to rule out extraneous factors which would themselves cause the phenomenon. For example, extreme care must be taken to hold the card steady. Any variation in the distance of the holes from the pupil of the eye will cause a

variation in the breadth of the overlapping area. Especially must care be taken that the card does not touch the lid of the eye, for movements of the ball of the eye and more particularly of the lid change the distance of the card from the eye. These movements are often unnoticed unless the observer is especially looking for them, and are frequently of sufficient range to cause a change in the size of the dispersion circles. Without a doubt the phenomenon, when it has occurred, has been, so far as the writer's experience is concerned, an artifact due to the conditions under which the observations were made. (3) Their technical demonstration by means of the ophthalmometer is, in the writer's opinion, just as strongly open to question. The writer criticizes this demonstration, however, with reluctance because of the meagerness with which they have described their method of working and observations. The following points, however, may be noted. (a) Working as they did, two images should have been observed, one reflected from the cornea, the other from the anterior surface of the lens.[32] Both images should have been very much alike, with the exception that the one reflected from the cornea should have been larger and more distinct. Nothing is said in the article, however, that would give evidence to the reader that more than one image was observed, or that the image described was actually reflected from the lens. But even if it were granted that the image observed was reflected from the lens, it would signify little, for the phenomenon described by them could have been caused just as well by involuntary eye-movements as by changes in the curvature of the lens. That is when the eye is accommodated, the anterior surface of the lens is hyperbolic in shape and varies in curvature considerably from point to point. A movement of the eye would, therefore, cause the rays of light forming the image to be reflected successively from points at which the surface had a different curvature. Each difference in curvature would give a difference in the size of the image reflected. Eye-movement would, therefore, produce the same effect in the size of the image as changes in the convexity of the lens. That is, movements of greater range would correspond in effect to the changes in convexity of greater magnitude, and, conversely, movements of lesser range to the changes in convexity of lesser magnitude. In fact, the phenomenon they describe is one of common

[32] An image reflected from the posterior surface of the lens might also have been observed. But since this image is inverted and is besides very indistinct, it may be considered as having no bearing on the discussion.

observation in case of the corneal image, and in this case no attempt has been made to ascribe it to recurrent changes in the curvature. For example, de Schweinitz, in his treatise on the diseases of the eye, says:[33] "Nothing is more common than to see the images of the mires [the mires correspond to the lights used by Heinrich and Chwistek] separate and overlap so that the apparent curvature of the cornea seems to change while under observation. The changes are due to slight movements of the eye which bring different portions of the cornea into view." We know that there are many involuntary eye-movements per minute even with the best control of fixation that can be obtained.[34] It seems more plausible, therefore, to attribute the phenomenon observed by Heinrich and Chwistek to the involuntary eye-movements which we know occur in abundance, than to use it as a proof of a new phenomenon, namely, the involuntary changes in the curvature of the lens, even if it be granted that the image from the lens was observed. At least, it may be said that Heinrich and Chwistek were not warranted in concluding as they did, without having secured any differential evidence to bear out their conclusion or without even having considered eye-movement as a causal factor. (d) Since the corneal image is known also to double and overlap, a rare opportunity was given to Heinrich and Chwistek, in making these observations, to compare the behaviour of the corneal image with that of the image reflected from the lens, if that really were the image they observed, and to determine by the presence or absence of coincidence in the two sets of changes, whether the doubling and overlapping of the images reflected from the lens has the same or a different cause from the doubling and overlapping of the images reflected from the cornea. Had both images really been observed or had the characteristic doubling and overlapping of the corneal image even been known to Heinrich and Chwistek, one can hardly conceive that their conclusions would have been drawn without recourse to this means of determining whether or not both sets of changes should be ascribed to a common cause. In short, judging from their report as it stands; from the fact that the ophthalmometer as it is ordinarily constructed and used is intended only for the observation of the corneal images, and that such a phenomenon as they describe would

[33] G. E. de Schweinitz: *Diseases of the Eye*, Philadelphia and London, 1902, 739.

[34] See C. E. Ferree: An Experimental Examination of the Phenomena Usually Attributed to Fluctuation of Attention, *Amer. Jour. Psychol.*, XVII., 1906, 113–115; also The Intermittence of Minimal Visual Sensations, *ibid.*, XIX., 1908, 83–112.

have been extremely difficult to observe in case of an image reflected from the lens; and from the fact that descriptions of similar behaviour on the part of the corneal image are given by other observers, the writer cannot help but think, without any wish to be hypercritical, that considerable grounds are given for suspecting that Heinrich and Chwistek have observed the doubling and overlapping of the corneal image which is commonly attributed by de Schweinitz and others to involuntary eye-movement.[35] Moreover, the crux of their argument is that they have actually observed a coincidence between the fluctuation of the visual stimulus and the changes in the adjustment of the lens. This, they contend, gives a certainty to their argument not yet attained in previous work on the problem. But even if the question whether or not it was a lens image that was observed be disregarded, it will be seen from the above discussion that is strongly probable that the coincidence they actually observed was between eye-movement and the fluctuation of the visual stimulus and not between changes in the curvature of the lens and the fluctuation of the visual stimulus.

(4) Their explanation of the effect of variation of area on the fluctuation of a visual stimulus could apply only to stimuli of very small area. Moreover, even in the case of very small areas the effect they got is just what might be expected as the result of increase of area either in terms of Loria's explanation of the fluctuation of stimuli of point area[36] or in terms of the writer's explanation: adaptation interfered with by eye-movement. They make two cases of their explanation of how changes of accommodation cause the fluctuation of stimuli of point area: (a) when the stimulus is a black point in a white ground and (b) when it is a white point in a black ground. In the former case the rays of light coming from the margin of the black point are not sharply imaged on the retina when the lens changes focus, hence they spread over the dark space on the retina corresponding to the black point. It is obvious that this spreading of the marginal light could blot out the dark space only in case the black stimulus were of very small area. Hence the explanation could not apply at all to stimuli of the size ordinarily used in the work on fluctuation. In the latter case the rays of light coming from the white point are not sharply imaged when the accommodation

[35] The writer leaves himself willingly open to correction on this point, however.

[36] See Heinrich and Chwistek: *op. cit.*, p. 60; also Stanislaw Loria: *Untersuchung über das periphere Sehen, Zeitschr. f. Psychol.*, XL., 1905, 160-186.

changes, and are spread over the surrounding dark space. Since strongly supraliminal stimuli were used, it is extremely doubtful whether even very small stimuli could be carried below the limen of sensation from this cause. Moreover, because strongly supraliminal stimuli were used and no attempt was made to control the intensity of the stimulus, an increase in the area of the stimulus would function for sensation as an increase of intensity.[37] Therefore, from this cause alone, according to the theories advanced either by Loria or by the writer, an increase of area would produce an increase in the phase of visibility. Even in case of stimuli of point area, then, the effect of increase of area described by Heinrich and Chwistek offers no differential argument in favor of the explanation advanced by them. Furthermore, the theory of fluctuation of attention was meant to apply only to stimuli of liminal or approximately liminal intensity. When such stimuli are used, an increase of area produces just the opposite effect. For example, working in 1906 with liminal stimuli ranging in area from .5 x .5 cm. to 15 x 15 cm., the writer found that an increase of area caused a decrease in the phase of visibility and a corresponding increase in the phase in invisibility. And in the experimental section of this paper it will be shown that the same effect is produced in case of liminal stimuli of very small area. In both of these cases care was taken to keep the stimuli liminal in order that an increase in the area of the stimulus would not produce an increase in the intensity of the sensation. (4) The fourth argument advanced by Heinrich and Chwistek has no differential value whatever. It was first used by Heinrich in 1898, as applied to stimuli of larger area.[38] A more intensive stimulus, he thinks, is not so liable to be blotted out by involuntary changes in accommodation. Therefore, he concludes, the more intensive is the stimulus the longer should be the phases of visibility and the shorter the phases of invisibility. It is obvious, however, that this result is just what should be expected from adaptation as a causal factor. It should be expected even were it held that fluctuation is due to instability of attention. In fact an increase in the phase of visibility and a decrease in the phase of invisibility would be the natural consequence of an increase in the

[37] We seem to have here a violation of one of the most fundamental principles in experimental procedure, namely, when it is wanted to determine the effect of a given factor, the effect of all other factors should, if possible, be eliminated from the results of the experiment.

[38] W. Heinrich: Zur Erklärung der Intensitätsschwankungen eben merklicher optischer und akustischer Eindrücke, *Bulletin International de l'Academie des Sciences de Cracovie*, Nov., 1898, 363-382.

intensity of the stimulus in terms of any theory that has yet been advanced to explain fluctuation.

(5) The writer is in some doubt as to what is meant by the fifth argument. "Befindet sich der Punkt, dessen Verschwinden man beobachtet, innerhalb des Akkommodationsbereiches der Linse, so beobachtet man nur das periodische Verschwinden desselben. Die Verhältnisse sind komplizierter, wenn man den Punkt ausserhalb des Fernpunktes aufstellt was beim myopischen Auge leicht ausführbar ist. In diesem Falle zeigt sich, dass der beobachtete Punkt, der jetzt nicht scharf gesehen wird, periodisch verschwindet, aber auch periodisch schärfer gesehen wird."[39] In the first place he cannot understand why the above result should be expected, were changes in accommodation present, for when the far point is actually reached the ciliary system should be completely relaxed. It is difficult then to see how the lens can be allowed to become any flatter, unless indeed it be held that the theory of accommodation commonly accepted for the human eye is incorrect. And in the second place, working under the conditions described by Heinrich and Chwistek, the writer has been unable to get anything that might be called three distinct and separate stages of clearness of his stimulus. Moreover, any stimulus of supraliminal intensity, fluctuating from any cause whatsoever and especially from causes purely retinal, would be apt to have, although not sharply defined, maximum, minimum, and intermediate degrees of distinctness. This the writer's observers were able to get at whatever distance the stimulus was put from the eye, but they were utterly unable to detect the three distinct and separate stages that are reported by Heinrich and Chwistek. Nor were they ever able to see the stimulus as clearly beyond the far point as they were at the far point or nearer than the far point. In short, there was never at this point what could be considered a norm of clearness which was succeeded either periodically or even at irregular intervals by a degree of clearness in excess of this norm. Continuing, Heinrich and Chwistek say: "Das lässt sich am besten durch folgendes Experiment illustrieren: Stellt man nicht weit ausserhalb des Fernpunktes des myopischen Auges als Objekt zwei Punkte, die so nahe liegen dass sie als ein Fleck gesehen werden, so beobachtet man, dass die Punkte periodisch auf kurze Zeiten getrennt erscheinen." The writer has not succeeded in getting this phenomenon when working beyond the far point with the myopic eye. It is, however, of common occurrence for any eye when the points are placed

[39] *Op. cit.*, 66.

at or slightly nearer than the limit of clear vision for these points and are regarded for any length of time. The points alternately blur into one and separate into two. In all probability both retinal and accommodation factors are involved in this result, but no definite estimate can be made of how much importance should be assigned to either until comparative records be made for subjects without lenses and for normal subjects. In the writer's opinion, however, the above experiment comes the nearest of any yet described by Heinrich and Chwistek to giving tangible evidence that involuntary changes in accommodation occur. But even to demonstrate clearly that these changes occur, would not prove that they are essential or even important factors in the fluctuation of minimal visual stimuli even of point area.[40] That they are not essential factors will be shown by the writer in the next section of this paper.

IV. Experimental

In this section of our paper we propose to show (1) that involuntary changes in accommodation are not essential or even important factors in the fluctuation of minimal visual stimuli of point area, and (2) that, identified by tests used by the writer in his earlier experiments, these fluctuations correspond just as closely to adaptation phenomena as they do for stimuli of larger area.

Probably the most convincing proof that one can offer that involuntary changes of accommodation are not essential to the fluctuation of stimuli of point area is the results obtained from aphakial subjects. Observations were made by the writer upon four aphakial subjects. They were all above sixty years of age, and three were above seventy. All of them had had the lenses removed from their eyes from 15-20 years before. Both the advanced age of the subjects and the long period that had elapsed since their lenses were removed favored the absence of any residual accommodation. To make sure of this point, however, they were each tested as follows. The subject's head was clamped in a head-rest and a card bearing letters of very fine print (3½ point type) was slid along a meter rod supported at the level of his eyes in the

[40] Lest it be thought that this experiment shows some coincidence between changes in accommodation and fluctuation, it may be pointed out that the cycle of changes experienced by the two points does not even include disappearance. The points merely blur into one and separate into two. That is, the only phenomenon cited by Heinrich and Chwistek that really gives any tangible evidence of involuntary changes in accommodation does not even occur in a series in which fluctuations are found.

median plane. The card was placed at his point of clearest vision as determined by the focus of his glasses and was moved both nearer and farther until just noticeable dimming took place. Every precaution was taken to secure accuracy. For one of the subjects the card could not be moved more than 2 mm. from the point of clearest vision without becoming less distinct. Very little more movement was required for any of the subjects. It may be safely said that all were practically without accommodation. Two of these observers were the same as were used by the writer in the earlier investigations made with stimuli of larger area. Opportunity was thus had to determine whether or not changes in accommodation play a more important rôle in the fluctuation of stimuli of point area than of stimuli of large area. So far as could be told from the records in both cases, they do not play a more important rôle. In cases of stimuli both of large and of very small area, the fluctuations occur for the aphakial subject with apparently no greater variation from the normal subject than is found from individual to individual with normal eyes.

Of the methods used in the former work to demonstrate that fluctuation is a phenomenon of the adaptation of the sense organ, only three were available for stimuli of point area. In the first of these the stimuli were made of different colors. Speaking of this method in the first paper[41] of the former series, the writer says, "Colors and grays were found to have an order of fluctuation times corresponding to their adaptation times. Four colors, red, green, blue, and yellow, gave very different fluctuation periods as compared with each other and with No. 27 Hering gray. The visibility times obtained were in the following order: red, green, blue, and yellow, the yellow being nearly four times as long as the red.

"The complete adaptation times for sheets of the same colors were found to have the same order of length and a rough correspondence as to ratio of length. Further, a striking fact came out with regard to the phases of invisibility. Since red, for example, has a shorter phase of visibility than green, one might naturally expect that its phase of invisibility would also be shorter than the phase of invisibility of green. The reverse, however, is true. Red has a longer invisibility than green, and this peculiarity is especially marked if one considers the proportionality between the phases, *i. e.*, the ratio invisibility: visibility. The same thing is true of the complementaries blue

[41] An Experimental Examination of the Phenomena Usually Attributed to Fluctuation of Attention, p. 86.

and yellow. Clearly, we cannot look for a central explanation of this peculiarity; but it seems just what we might expect of adaptation from the standpoint of the compensation theory. The recovery process for the red is the green process. The green process is longer and seemingly more tenacious than the red, as is shown by the adaptation experiments proper, and is further borne out by the longer duration of the green after-image. A similar relation obtains in the blue-yellow process." In the earlier work, the stimuli were gotten as follows. Squares of the color of the size that was wanted were pasted on a gray of the brightness of the color. The stimulus was rendered liminal by letting the light pass from the colored paper through a sheet of milk glass, matt on one side, placed at such a distance from the color as to render its intensity liminal. The intensity was easily regulated by slight changes in the distance of this glass from the colored paper. The light reflected from the colored papers could not be used, however, for stimuli of point area, because the milk glass mentioned above had to be used to reduce the intensity of the stimulus and it was impossible to get this glass thin enough to give noticeable color with stimuli of point area. Light was transmitted through color-filters instead. The stimulus was gotten as follows. A hole was pricked through a gray cardboard with a fine needle and covered with one or more layers of colored gelatin. In front of the card, in contact with it, was placed the sheet of milk glass, matt on one side. The hole was illuminated by a row of lights placed behind the cardboard, normal to its surface, at a distance sufficient to render the stimulus liminal. By this arrangement a just noticeable point of color was presented to the observer seated in front.

A stimulus given by reflected light has always yielded more differential results in former experiments with the method of colors than a stimulus by transmitted light. This is probably due to the fact that we were able to get from the former type of stimulus more color in proportion to the white light present, thus better bringing out the color differences in the liminal stimuli. The poorer method had to be used, however, because as stated above milk glass with one surface matt could not be obtained thin enough so that a point of colored paper pasted upon a background of equal brightness could be seen through it.[42]

[42] In case of the colored papers the liminal stimulus and surrounding field were of the same brightness, because the paper giving the stimulus was pasted on a gray of the brightness of the color. The only effect of the milk glass in front was to change the general scale of brightness of color and surrounding field. No brightness inequality was produced.

The registration of results was secured by means of a Ludwig-Baltzar kymograph, a telegraph key and an electromagnetic recorder, a Jaquet chronograph (set to seconds), and a lamp rheostat to cut down the current from the lighting circuit. All of this apparatus was screened from the observer by means of a sliding curtain. The work was done in a long room with the windows all at one end. Thus cross lights, unequal illumination of the background, etc., could be avoided. The illumination of the room was kept fairly constant by means of thin curtains covering the windows.[43] The observer sat with his back to a high window and his head in a head-rest fastened to the edge of a long table, along which the frame bearing the stimulation apparatus was moved as required. The time used throughout was 1 sec. The following results were obtained. As in the earlier work with stimuli of a larger area, red showed a shorter phase of visibility and a longer phase of invisibility than green; and blue, a shorter phase of visibility and a longer phase of invisibility than yellow. In spite of the poorer method we were required to use, the results obtained were almost as strongly marked as they were when the same method was used with stimuli of a larger area. These results have been verified at the time this work was done and since by a large number of observers practiced and unpracticed. The results of three observers chosen as typical will be reported here. Tables I-III have been compiled from these results.

TABLE I

OBS. C.—Fluctuation with stimuli of the four principal colors of point area showing that the phases of visibility and invisibility have the characteristic adaptation and recovery peculiarities of these colors just as they have with stimuli of larger area.

Stimulus	Vis.	M.V.	Invis.	M.V.	Vis. ÷ Invis.	Invis. ÷ Vis.	Period
Red	4.36	.79	1.68	.65	2.585	.385	6.04
Green	5.30	.93	1.13	.42	4.690	.213	6.43
Blue	7.75	1.12	1.41	.71	5.496	.181	9.16
Yellow	13.10	1.76	1.32	.59	9.925	.100	14.42

In the case of the stimulus by transmitted light, this result was not so effectively secured because of the greater difficulty of equating the point of light and the surrounding field.

[43] To keep the illumination constant presupposes a means of measurement. At the time the writer had at his command no means of measuring the illumination of a room by daylight. For a method of

TABLE II

Obs. G.

Stimulus	Vis.	M.V.	Invis.	M.V.	Vis. ÷ Invis.	Invis. ÷ Vis.	Period
Red	5.1	1.12	1.26	.54	4.047	.247	6.36
Green	7.43	1.54	1.14	.37	6.517	.153	8.57
Blue	10.9	1.59	1.46	.52	7.466	.133	12.36
Yellow	Did not fluctuate at all during period of observation.						

TABLE III

Obs. Ca.

Stimulus	Vis.	M.V.	Invis.	M.V.	Vis. ÷ Invis.	Invis. ÷ Vis.	Period
Red	1.82	.59	1.62	.32	1.123	.890	3.44
Green	2.82	.53	1.27	.36	2.243	.450	4.09
Blue	4.15	.55	1.70	.35	2.441	.409	5.85
Yellow	5.22	.86	1.52	.51	3.434	.291	6.74

In the second test strips of colored paper of the breadth of a point and 5 cm. in length were used.[44] They were pasted on a gray background of the brightness of the color in each case and were observed as liminal color on the matt surface of the milk glass placed in front. They were arranged first with their longer dimension in the vertical plane, then in the horizontal plane. The former arrangement favored a maximal disturbance of adaptation for observers having the greater range and frequency of eye-movement in the horizontal plane, and gave with these observers in the fluctuation experiments a corresponding increase in the phase of visibility and decrease in the phase of invisibility. Conversely, the latter arrangement favored a minimal disturbance of adaptation for these observers and gave a corresponding decrease in the phase of visibility and increase in the phase of invisibility.

doing this, see C. E. Ferree and Gertrude Rand: An Optics-Room and a Method of Standardizing Its Illumination, *Psychol., Rev.*, XIX., 1912, 364-373.

[44] In this test we were able to use colored paper because strips, although only of the breadth of a point, could be seen when 5 cm. long through the sheet of milk glass we used.

For each observer careful records were made of the frequency and range of movement and the total time the eyes were moving according to the methods described in the former papers.[45]

Speaking of this test in the first paper of the former series, the writer says, pp. 84-90, "A more direct experimental confirmation than was afforded by the method of variation of areas of this view that eye-movement interferes with the course of adaptation and is also the conditioning factor for the wide range of variability found in the phases of visibility and invisibility in the fluctuation experiments, is given by the following results. An examination of average frequency of eye-movement in the horizontal and vertical planes during fixation showed that three of our observers had a marked excess in both frequency and range in the horizontal, while the fourth had an excess of frequency in the vertical, but of range in the horizontal plane. This appeared to mean that for three observers, there was greater change of stimulation, and consequently greater relief for the adapted elements, in the horizontal than in the vertical direction; while the reverse was true, though probably to a lesser degree, for the fourth. To test this interpretation, stimuli longer than broad were used, *e. g.*, slips of paper 5 mm. x 40 mm. When these were placed with the longer dimension vertical, the shorter dimension would fall in the direction of greater unsteadiness of fixation for the three observers who had the excess of eye-movement in the horizontal plane. Consequently, a maximal interference with adaptation for these stimuli would be obtained, and one might expect an increase in the phase of visibility and a decrease in the phase of invisibility. On the other hand, if the longer dimension were placed in the horizontal and the shorter in the vertical plane, the minimal interference possible for these stimuli would be secured, and a decrease in the phase of visibility and an increase in the phase of invisibility should ensue. For the fourth observer with the stimulus arranged as described above, the reverse should be true, but probably not in so marked a degree, since his range was greater in the horizontal, which fact to a certain extent would counteract the effect of frequency. . . . . That these methods of arrangements of stimulus caused a marked change in the phases of visibility and invisibility for each

[45] See An Experimental Examination of the Phenomena Usually Attributed to Fluctuation of Attention, 113-115; and The Intermittence of Minimal Visual Sensation, 84-87.

observer will be seen by inspecting the Tables. Indeed the correspondence between the quantities: $\frac{Visibility \div invisibility}{visibility^1 \div invisibility^1}$, and $\frac{frequency}{frequency^1}$, is much closer than was anticipated."[46]

The results for the strips of point breadth are given in Tables IV-VI. For all the observers whose results are given in these tables, both the range and frequency of eye-movement were greater in the horizontal than in the vertical plane.

The third test was based upon the fact that the time required for a colored stimulus to adapt depends to some extent upon the surrounding field.

The question of what is meant by adaptation is logically raised here; among the followers of the Hering theory, it has come to mean, apparently, simultaneous induction, and Aall, reviewing the writer's first article,[47] assumes that that is what is meant by adaptation in that

TABLE IV

Obs. H.—Fluctuation with horizontal and vertical arrangement of the stimulus. Showing how arrangements that favor maximal and minimal interference with adaptation affect the phases of visibility and invisibility. Stimulus 3 mm. x 50 mm.

Stimulus	Arrangement	Vis.	M.V.	Invis.	M.V.	Vis. ÷ Invis.	Invis. ÷ Vis.	Period
Red....	Vertical....	2.95	.64	1.68	.31	1.756	.569	4.63
"	Horizontal..	1.08	.23	2.29	.40	.471	2.120	3.37
Green..	Vertical....	4.04	.72	1.45	.26	2.786	.358	5.49
"	Horizontal..	1.69	.38	1.76	.37	.960	1.041	3.45
Blue....	Vertical....	5.40	.86	2.03	.51	2.660	.376	7.43
"	Horizontal..	2.51	.49	3.10	.46	.806	1.235	5.61
Yellow..	Vertical....	6.99	.97	1.45	.29	4.82	.207	8.44
"	Horizontal..	3.55	.72	2.10	.56	1.928	1.690	5.65

[46] For a more complete understanding why arranging the shorter dimension of the stimulus in the direction of the greatest eye-movement causes relatively long phases of visibility and short phases of invisibility; and conversely arranging the longer dimension of the stimulus in the direction of greatest eye-movement causes relatively short phases of visibility and relatively long phases of invisibility, see The Intermittence of Minimal Visual Sensations, 112-129; and The Streaming Phenomenon, 484-494.

[47] *Zeitschr. f. Psychol.*, XLIII., *Abt.* 2, 1906, 456-457.

TABLE V

OBS. R.

Stimulus	Arrangement	Vis.	M.V.	Invis.	M.V.	Vis. ÷ Invis.	Invis. ÷ Vis.	Period
Red....	Vertical....	3.60	.75	1.46	.24	2.548	.405	5.06
"	Horizontal..	1.14	.28	3.40	.75	.335	2.982	4.54
Green...	Vertical....	4.30	.62	1.39	.19	3.093	.323	5.69
"	Horizontal..	2.42	.47	2.36	.37	1.025	.975	4.78
Blue....	Vertical....	4.93	1.08	1.68	.34	2.933	.340	6.61
"	Horizontal..	2.95	.54	4.53	.89	.651	1.535	7.48
Yellow..	Vertical....	6.61	1.23	1.46	.31	4.527	.220	8.07
"	Horizontal..	3.26	.79	2.75	.49	1.189	.843	6.01

TABLE VI

OBS. G.

Stimulus	Arrangement	Vis.	M.V.	Invis.	M.V.	Vis. ÷ Invis.	Invis. ÷ Vis.	Period
Hering gray No. 27	Vertical....	8.48	1.32	2.03	.39	4.177	.239	10.51
"	Horizontal..	3.14	.71	3.21	.56	.978	1.022	6.35
Red....	Vertical....	5.09	1.12	1.84	.29	2.766	.361	6.93
"	Horizontal..	1.65	.38	3.61	.78	.457	2.187	5.26
Green...	Vertical....	7.42	1.29	1.94	.41	3.824	.261	9.36
"	Horizontal..	2.52	.48	2.98	.48	.845	1.182	5.50
Blue....	Vertical....	8.99	1.47	2.84	.39	3.165	.315	11.83
"	Horizontal..	3.88	.84	5.99	.97	.647	1.542	9.87
Yellow..	Vertical....	11.49	1.97	1.90	.32	6.047	.165	13.39
"	Horizontal..	4.84	.97	3.86	.89	1.254	.797	8.70

article. The writer, however, by no means believes that the tendency of a color to lose its saturation on prolonged exposure to the eye or of all grays to become mid-gray is due entirely or even to any considerable extent to simultaneous induction. He grants an influence to the surrounding field when there is a surrounding field, and is at present making a quantitative study of that influence, but it is obvious that the influence of the surrounding field can have no part in the phenomenon called general adaptation, for in that case the whole retina is stimulated by the same kind of light. It can apply to local adapta-

tion alone and even in local adaptation it cannot be considered a factor of primary importance. In 1838-1840 the loss of saturation experienced by a color on prolonged exposure to the eye was explained by Fechner[48] as due to the exhaustion or fatigue of the retinal elements. This explanation was adopted by Helmholtz, and became a feature of the Young-Helmholtz theory. Hering,[49] however, following a suggestion made by Godart, 1776,[50] and elaborated by Plateau, 1833-1835,[51] chose rather to consider the retina compensating in function. A compensating retina, it is obvious, should not exhaust. Hering bore himself out in this general position by claiming that the eye is ordinarily exposed to stimulation by white light from 15-18 hours during the course of a day, and yet at the end of that time it has not noticeably lost in its sensitivity to white light.

Hering himself apparently has not based his claim on experimental evidence. At least he neither offers results of his own nor quotes from the work of others. His conclusion seems to be drawn wholly from general observation. He says (Ueber Ermüdung und Erholung des Sehorgans, *Arch. f. Ophthal.*, XXXVII., 1891, (3), p. 2): "Anderseits ist es eine bekannte Thatsache, dass wir des Abends nicht merklich schlechter sehen als des Morgens und dass dies auch dann noch der Fall ist, wenn dem Tage eine in hellen Raumen durchwachte Nacht und ein neuer schlafloser Morgen folgt. Also einerseits fortwährende Ermüdung und zwar eine so schnell—vor sich gehende, dass schon nach einer wenige Secunde währenden—Fixierung eines weissen Objects auf dunklem Grunde sich die Folgen der "Ermüdung" durch ein deutliches negatives Nachbild verrathen, und anderseits trotz solcher fortwährenden raschen Ermüdung keine merkliche Beeinträchtigung des Lebens selbst bei tagelanger Belichtung der Netzhaut." He contends (p. 1) that according to the theory of fatigue, advocated by Helmholtz and Fick, this should not be. During an exposure of several hours to white light, the eye never has a chance completely to recover, hence should become from beginning to end of the period progressively more fatigued.

This conclusion is not at all in agreement with experimental results obtained by C. F. Müller (Versuche über den Verlauf der Netzhautermüdung. Diss. inaug., Zürich. 1866), for example, who from the results of his tests of the loss of sensitivity of the eye to white light from morning to night, concludes: "Am Abende erscheint der Retina irgend ein Object nur in 0.49 derjenigen Helligkeit, in welcher es ihr am Morgen erschienen wäre." Moreover, he found that the shape of the curve of fatigue undergoes a very decided change during the course of the day. Aubert also disagrees with Hering. He says

[48] G. T. Fechner: *Pogg. Ann.*, XLIV., 1838, 221, 513; XLV., 1838, 227; L., 1840, 193, 427. The theory was conceived earlier by Scherffer (*Abhandlung von den zufälligen Farben*, Wein, 1765; also *Journal de Physique de Rozier*, XXVI., 175, 273), who explained the negative after-image by the conception that the retina is diminished in sensitivity by fatigue produced by previous stimulation.

[49] Ewald Hering: *Zur Theorie vom Lichtsinne*, 1874; *von Graefe's Archiv*, XXXVII., 1891, (3), 1, and 1892, XXXVIII., (2), 252.

[50] de Godart: *Journal de Physique de Rozier*, VIII., 1776, (1), 269.

[51] Plateau: *Ann. de Chimie et de Physique*, LIII., 1833, 386; LVII., 1835, 337; *Pogg. Ann.*, XXXII., 1834, 543. More fully in *Essai d'une théorie générale, etc. Mem. de l'Acad. de Belgique*, VIII., 1834.

(Moleschott's Untersuchungen, VIII., 1862, 251; see also Beiträge zur Physiologie der Netzhaut. *Abhandlungen der Schlesischen Gesellschaft,* Breslau, 1861, 39): "Es erscheint mir also aus obiger Bemerkung hervorzugehen, dass *im Laufe des Tages durch die Einwirkung des Lichtes die Empfindlichkeit unserer Retina fortwährend abnimmt,* so dass wir am Abende weniger empfindlich gegen Licht sind, als des Morgens." Moreover, without supporting evidence either from general observation or from experiments on color, in fact in complete disregard of this evidence, Hering, as he has done in many other cases in his work on the optics of color, has generalized with regard to the retina's response both to white light and to colored light from the results of observations with white light alone. For example, it is scarcely necessary to point out that the eye cannot be exposed from 15-18 hours to colored light without loss of sensitivity to color. Without dwelling further, however, on the evidence for and against a compensation theory, it will be sufficient for our purpose here to point out that if one were to hold to a compensation theory in the Hering sense, it would be necessary for him to seek some other explanation than exhaustion for the loss of sensitivity of the eye, apparent or real, to color or brightness. Hering apparently conceives that this happens only in case two surfaces of different quality are juxtaposed, and then all that takes place is that each is induced over the other and the qualitative difference between the two tends to disappear. There is, then, no real loss of sensitivity of the eye to either. Both become alike because by induction they are mixed to equality. The following objections may be offered to the explanation. (1) As stated before, it cannot apply to general adaptation. Yet it is well known that the eye loses its sensitivity to color when the whole retina is stimulated by that color, in fact more rapidly than when only a part is stimulated, except perhaps in case of certain combinations of color and surrounding field. (2) It can apply to local adaptation only in case the two fields juxtaposed both belong to the brightness series. For example, when the eye is exposed for some length of time to a white surface contiguous to a black or a light gray to a dark gray, the lighter surface is observed to darken and the darker to lighten. This might be explained by the mixture of the two qualities by induction. The evidence afforded by the observation, however, is not at all differential, for the phenomenon may be explained just as well by exhaustion. A different situation entirely is presented, however, when the two contiguous surfaces are colored. In this case there is very little in the phenomenon that could by the most favourable interpretation be construed as a mixture to an intermediate color quality. For example, when red and blue are stared at in juxtaposition, we should expect, in terms of Hering's explanation, both surfaces to become purple with no more loss of saturation than would be attendant upon distributing each color uniformly upon both surfaces. This, however, is not at all what takes place. The prominent effect is loss of saturation. The two surfaces tend to become alike for the most part only because both tend towards gray. The blue, it is true, does acquire a tinge of violet, but it does this as the result of adaptation even when red is not juxtaposed. It probably does become slightly more reddish by being alongside the red, but the evidence of induction is not great. The red, likewise, may be modified a little by being alongside the blue, but the effect is even less noticeable than it is for the blue. Similar results are gotten with

green and yellow. In case the colors juxtaposed are complementary colors, the results of induction should be towards a cancellation to gray. But again the tendency towards gray which is actually observed affords no differential evidence for this theory of induction, because the shift towards gray can be explained just as easily in terms of the exhaustion theory. And that induction can have little to do with the phenomenon may be shown by the facts (1) that the tendency would have been towards gray had the whole retina been stimulated by one of the colors alone, and (2) that so far as can be told, the process is hastened little, if any, by the juxtaposition of the two colors. In the Lichtsinne, 1878, pp. 36-37 Hering describes the experiment upon which he bases his explanation of adaptation in terms of simultaneous induction. His device for stimulating the eye consists of a white and black surface juxtaposed. No attempt is made to extend the experiment to color. Moreover, in drawing his conclusions, no heed whatever is given to what would happen were the whole retina stimulated by light of one quality. This is a truly remarkable instance of a broad generalization made from a slender basis of fact.

The writer, then, does not wish it to be understood that he explains the fluctuation of minimal visual stimuli in terms of simultaneous induction. He has called this fluctuation a phenomenon of the adaptation and recovery of the sense organ, meaning by adaptation here, as in the original article, the progressive loss of sensitivity of the eye to colored and to colorless light caused by prolonged exposure. Just what the factors are in adaptation, will be made the subject of a further paper. They vary under different circumstances. In case of local adaptation, simultaneous induction is one of the factors, and in certain especial cases it may exert considerable influence, as is recognized in the test described above; but to make it the sole cause of the adaptation of the eye to its stimulus seems to the writer, in the face of the experimental evidence, to be little short of absurd.

In the earlier experiments it was found that by keeping the surrounding field constant and varying the stimulus, or conversely, by keeping the stimulus constant and varying the surrounding field, a difference in the period of fluctuation was obtained, showing itself chiefly in the phase of visibility. The same thing held in the recognized adaptation experiments. The variations in the phases of visibility and invisibility that were produced in the one, were produced in the other; the only departure from precise correspondence being that the differences were more marked in case of the recognized adaptation experiments, as would be expected from the longer duration of the process. The old series of Hering papers was used both in these experiments and in the experiments with stimuli of point area because combinations more favorable to rapid adaptation could be found in this series. Some of the combinations most favorable were the vermilion of the series on the blue-green, and the vermilion upon Hering gray No. 27; and some of the most unfavorable combinations were dark

red on yellow, and dark blue on yellow. The combinations favoring rapid adaptation gave in the fluctuation experiments a short phase of visibility and a long phase of invisibility, and conversely, the combinations unfavorable to rapid adaptation gave long phases of visibility and short phases of invisibility. Although the writer had carefully determined in an earlier experiment with large areas which were the favorable and which the unfavorable combinations, still in order to make the correspondence between fluctuation and adaptation still more complete, in cases of stimuli of very small area both adaptation and fluctuation experiments were conducted in the present study. As was the case in the earlier experiments, the advantages of a stationary stimulus and surrounding field had to be sacrificed in these experiments, because the use of the milk glass to reduce the saturation of the stimulus, as was done when a stationary system was used, would also have reduced the saturation of the color in the surrounding field. This would not have been desirable for the purpose of the experiment. Accordingly, the Masson disc with the broken radius of point breadth was substituted for the stationary system. In case of the adaptation experiments, a point of color of full intensity was pasted upon the various backgrounds and observed at the proper distance. In conducting this adaptation series with stimuli of point area, we were not only getting the results needed for comparison in our fluctuation series, but by using stimuli of full intensity, we were applying our test under precisely the same conditions used by Heinrich in his fluctuation experiments. In both cases the effect of the favorable and unfavorable combinations was plainly marked in the results. For the results of these experiments see Tables VII-X.

TABLE VII

OBS. R.—Showing that combinations that influence adaptation time correspondingly influence fluctuation for stimuli of point area just as they do for stimuli of larger area. Fluctuation series, stimulus-ring 0.3 mm. broad and of liminal intensity.

Stimulus	Background	Vis.	M.V.	Invis.	M.V.	Vis. ÷ Invis.	Invis. ÷ Vis.	Period
Red....	Blue.....	4.258	.81	1.930	.83	2.220	.450	6.215
Red....	Orange.....	6.041	.98	1.327	.59	4.552	.219	7.368
Red....	Yellow-green	6.10	.91	.933	.32	6.538	.152	7.033
Yellow..	Red.....	9.166	1.10	1.125	.26	8.147	.122	10.291

TABLE VIII

OBS. B.

Stimulus	Background	Vis.	M.V.	Invis.	M.V.	Vis. ÷ Invis.	Invis. ÷ Vis.	Period
Red....	Blue.....	3.30	.82	2.871	.88	1.149	.870	6.171
Red....	Orange.....	5.083	.94	2.416	.75	2.103	.475	7.499
Red....	Yellow-green	4.125	.59	1.540	.64	32.678	.373	5.665
Yellow..	Red.....	6.230	.94	1.050	.32	5.933	.168	7.28

TABLE IX

OBS. R.—Showing that combinations that influence adaptation time correspondingly influence fluctuation for stimuli of point area just as they do for stimuli of larger area. Adaptation series, stimuli of point area and of full intensity.

Stimulus	Background	Vis.	M.V.	Invis.	M.V.	Vis. ÷ Invis.	Invis. ÷ Vis.	Period
Red...	Blue.....	13.0	1.09	2.577	.76	5.044	.198	15.577
Red...	Orange.....	11.323	.90	1.112	.58	10.182	.098	12.435
Red...	Yellow-green	11.032	.87	.781	.32	14.122	.070	11.813
Yellow.	Red.....	200.	No	fluctu	ation.			

TABLE X

OBS. B.

Stimulus	Background	Vis.	M.V.	Invis.	M.V.	Vis. ÷ Invis.	Invis. ÷ Vis.	Period
Red...	Blue.....	5.761	.59	5.833	.654	.987	1.012	11.594
Red...	Orange.....	6.636	.98	4.723	.76	1.405	.711	11.359
Red...	Yellow-green	10.751	1.05	4.854	.97	2.215	.451	15.605
Yellow.	Red.....	200.	No	fluctu	ation.			

V. Conclusion

In conclusion the following points may be reviewed. (1) The work offered by Heinrich and Chwistek in support of the accommodation theory for the fluctuation of stimuli of point area was done with stimuli of full intensity. In using stimuli of this intensity Heinrich and Chwistek have created for themselves a special problem. The doctrine of fluctuation of attention has never been applied to the fluctuation of stimuli

strongly supraliminal in intensity. (2) Their strongest and most direct argument for the accommodation theory is their claim of having directly demonstrated a coincidence between involuntary changes of accommodation and fluctuation. The validity of this claim, however, rests primarily upon whether or not they have given a valid demonstration of the involuntary changes in accommodation. Their demonstration of involuntary changes in accommodation is strongly open to question. Employing 124 observers, the writer has been unable in a single case to make their popular demonstration work when care was taken to rule out extraneous factors which would themselves cause the phenomenon. And their technical demonstration with the ophthalmometer is in terms of a phenomenon which is described by de Schweinitz and others as one of the common phenomena of ophthalmometry due to eye-movement. The coincidence, then, which they claim to have observed between the fluctuation of stimuli of point area and changes in the curvature of the lens, is in all probability a coincidence between eye-movement and fluctuation. (3) Moreover, none of the evidence they have offered as indirectly proving the accommodation theory can be considered in any sense differential. All of it can be explained just as easily either in terms of the writer's adaptation theory or in terms of Loria's theory for the fluctuation of stimuli of point area. Some of it can even be explained in terms of any theory that has yet been advanced to account for the fluctuation of minimal visual stimuli. (4) The fluctuation of stimuli of point area presents no especial case. For (a) involuntary changes in accommodation are not an essential factor in these fluctuations. They take place for aphakial subjects apparently just as readily as for subjects with normal eyes. And (b) identified by the tests used by the writer in his earlier work these fluctuations correspond just as closely to adaptation phenomena as do the fluctuations of stimuli of larger area. (5) The fluctuation of minimal visual stimuli whether of large or small area is a phenomenon of the adaptation and recovery of the sense organ. And by adaptation is meant the progressive loss of sensitivity to colored and colorless light caused by prolonged exposure of the eye to these lights. It is not simultaneous induction. Simultaneous induction can be considered only as a minor factor in the adaptation of the eye to its stimulus.

THE CHARACTERISTIC FORM ASSUMED BY DREAMS

ELIOTT PARK FROST, Yale University

It is as rare for a dream of any length to follow a single path or direction as for a train of waking thought to do so. A pulse of attention, fatigue, or a new moment of interest severally enters to shift conscious processes into new channels, and a similar shifting takes place in dreams. In this latter case, however, a study of the explicit phenomena of dreams has shown interesting peculiarities that call for further description. These phenomena appeared, somewhat as by-products, in the investigation of the implicit factors in dreams, conducted through a period of several months.

Attending to dreams aggravates them, fortunately for the experiment if not for the experimenter. The *Aufgabe* appears to exert an influence during sleep-hours, so that by attending for a long period to dreams one comes to be a light sleeper and a prolific dreamer. In time, literally *trains* of dreams come readily to mind and can be for the most part transcribed to paper in considerable detail. It is perhaps only under these conditions, when dream-material is abundant, that the true characteristic or type-form of the dream series appears, and may for this reason have escaped general notice.

Unless the conditions be unusual, as when some fixed idea monopolizes the dream,—a fatigue phenomenon,—or when the duration of the dream be actually brief, a dream is found to be constituted of several *dream-phases,* or *motifs,* distinct one from the other, in visual and other imagery, in characters and in situations, but linked together by some associative nexus, usually of an imaginal, but occasionally of an affective sort. Something from one dream persists into and 'sets off' the succeeding dream-phase. By means of this relational element, antecedent dreams are often recalled at a time considerably subsequent to the recall of the end-dream or dream at the waking moment. Or, again, chains of dreams are pictured at once *in situ* upon awaking. In any case there appear to be characteristic schemata to which the content conforms perforce, and which therefore must be considered

in evaluating even these implicit (Freudian, and other) factors in dreams.

What these schemata are will appear if we take an actual dream-manifold and analyze it. Such an one is the following, taken from notes made at the moment of awaking (abbreviated):—

". . . . . standing on a side-hill with a friend. Note in distance an ENORMOUS AIR-SHIP approaching at FIERCE SPEED. It SHOOTS by overhead, circles, turns over and over gracefully . . . . . the movements GROW SLOWER and it comes to earth. I . . . . . make conversation with the pilot and passenger, the latter discussing the fine points of the air-craft with me. . . . . By this time the air-ship has become greatly REDUCED IN SIZE to the proportions of an ordinary MONOPLANE . . . . . the passenger sits in a LITTLE HAMMOCK in the rear near the propeller. . . . ."

There is now a shift of scene, with new surroundings, new characters:—

". . . . .Am beside a BROAD lake near a LARGE hotel watching the arrival of guests. In the foreground stand some PAINTERS' LADDERS on a pair of wheels, and I call out: 'Tell the carpenters to come for their ladders'. . . . . have a sense of saying something very strategic as though concealing the fact that these were not really ladders but something else (?). . . . . Now . . . . . go down to the water to greet arriving guests. . . . . The scenery SHRINKS . . . . . the guests become REDUCED IN NUMBER to three intimate friends whom I now undertake to introduce one to another . . . . . some names are forgotten and embarrassment ensues. . . . ."

Here, again, occurs a complete shift of scene.

"Am now in a GREAT FOREST, ENORMOUS TREES, CROWDS of gaily-dressed people, horses, etc. . . . . Seem to be directing the presentation of a pageant . . . . . movement of figures to and fro as I gesture and command. . . . . Gradually the figures grow SMALLER, the field as visually imaged is RESTRICTED . . . . . I now appear to be watching dolls perform on a cardboard stage in some ROOM or other."

Here is a dream manifold in three distinct parts. As a matter of fact the separate parts were recalled upon waking in reverse order, the last scene in the forest being the first recalled, then the lake-scene, and lastly, while the other two dreams were being put to paper, the first dream of the air-ship.

was brought back to mind. In this series of dreams at least four phenomena are significantly illustrated:

(1) A given dream-episode is launched abruptly, "full-blown." At its initiation all the elements of imagery are maximal; things then seem brightest, largest, widest, highest, loudest, quickest, etc. So in the dream appears the 'ENORMOUS AIR-SHIP,' 'FIERCE SPEED,' 'BROAD LAKE,' 'GREAT FOREST,' 'CROWDS OF PEOPLE,' etc., at the *beginning* of the respective episodes.

(2) Upon the appearance of such a congeries, a process of condensation begins at once. Things pale, grow small, are suppressed or disappear altogether, sometimes retaining a kind of form with loss of substance, as it were. Meaning and content are dimmed or changed. So, above, the 'MOVEMENTS GROW SLOWER,' 'AIR-SHIP REDUCED IN SIZE,' 'LITTLE HAMMOCK,' 'SCENERY SHRINKS,' 'GUESTS REDUCED IN NUMBER,' 'FIGURES GROW SMALLER,' 'PEOPLE BECOME DOLLS,' and a 'ROOM' takes the place of outdoors.

(3) As before suggested some one factor in a given episode persists, becoming the nucleus of the succeeding episode. The latter is abruptly initiated and incorporates the nucleus in a new complex. Thus the monoplane of (1) becomes the carpenters' ladders of (2), and the presenting of friends in (2) becomes the directing of a pageant in (3). In no two successive episodes of a dream-series was the second or later episode wholly independent of the episode preceding. However great the apparent gap between the two, some point of community could always be found.

(4) Finally in certain dreams imaginal elements, while still vivid, may play a rôle relatively subordinate to an affective element. In case a strong affective element, *e. g.*, FEAR, is present, a temporal summation of the dream-emotion may occur, persisting through successive episodes to culminate in arousing the sleeper. In such cases the affective element often itself becomes the link of connection between episodes, when perhaps no other traceable element can be found. This is relatively rare however.

* * * * * * * * *

Now is it possible to go beyond the mere description of these phenomena? Can they be explained? One or two suggestions may be adventured:

(1) There appear to be rhythms or phases in the continuous dream analogous to attention waves.

(2) These rhythms differ from the body-rhythms of breathing, heart-beat, etc., in the abruptness with which they are initiated. In this respect they are analogous to a new moment of attention, and suggest an 'explosion' of energy rather than a pendular wave.

(3) A residuum of energy from one phase acts as a stimulus in the release of energy for a succeeding phase.

(4) Finally, each phase may contribute an increment of energy to vaso-motor centres, where it does not find outlet at once but is gradually summated, until there is a sudden awaking, marked by a cry, start, forced breathing, perspiration, or similar physiological accompaniment; these motor phenomena serving as channels of drainage for the sympathetic system.

SUPPRESSION AND SUBSTITUTION AS A FACTOR IN SEX DIFFERENCES[1]

By M. E. Haggerty and E. J. Kempf

Among the factors which influence the rate and efficiency of mental association is the tendency toward suppression and substitution. That the strength of this tendency constitutes a distinct sex difference, being stronger in women than in men, is indicated by the results of tests which are here reported.

The subjects were 12 women and 16 men, students in a psychological laboratory. Each had had an elementary course in psychology and had worked a term and a half in the laboratory. They were, therefore, especially good subjects.

The tests used were selected from the Woodworth and Wells association tests. They fall into two groups. The first group included the two cancellation tests, the two naming tests, the substitution test and the two directions tests. All the tests were made under the supervision of the director of the laboratory. In each series of the first group, the first test was given to a student by the director. This student was then instructed how to perform the test and he gave it under supervision to the other 27 subjects. The logical relation tests, which constituted the second group which were given by Dr. Kempf included the opposites test, the verb object test, the action-agent test, the attribute-substance test, the subordinate concept test, the agent-action test, and a reverse opposites test. The latter was made up by selecting the true opposites of the Woodworth and Wells list of forty-opposites. This test was given by Miss Mitchell under Dr. Kempf's direction. In the logical relation tests each stimulus word was pronounced by the experimenter and the subject responded orally. The oral reaction word and reaction time were individually recorded.

In the logical relation series the time was taken with a stop watch. The same was true of the other series excepting where the time was long. In those cases an ordinary watch was used. The same method was used throughout each series so that within a given series each test had the same degree of accuracy.

[1] From the Psychological Laboratory of Indiana University.

The tests were first undertaken for practice work. Care was taken from the first, however, to see that all the conditions were thoroughly standardized so that the results would be reliable. When the first seven series were completed it seemed worth while to gather the results into a table separating the men and women into different groups. When this was done there appeared a distinct sex difference in favor of the women. This difference occurred not only as concerns the whole set of tests but it appeared in each separate series. The per cent of difference ranged from 8% in the Form naming test to 15% in the Cancellation of 2 test.

In discussing these results with the subjects it was suggested that the women might have made more errors than the men and thus increased their time. It was possible at the time to make an examination of the results of the cancellation tests and of the easy directions test only. In both of these there were more errors for the men. It does not seem probable therefore that the superiority of the women was due to careless work.

A second suggestion was that the greater length of time occupied by the men was due to wide individual variation. This would be individual rather than sex difference. In figuring the average for the whole group of tests it was found that the M.V. for the men was slightly greater than for the women, 13.5″ for the latter as against 16.5″ for the former. The number of wide variations, however, were about equally distributed between plus and minus variations so that we do not seem to find the explanation here.

The apparent sex differences might be attributable to the women being a more highly selected group than the men. In order to test this hypothesis the grades of all were obtained from the University office. All the grades in all the subjects for all the time the students were in college were considered. The average for the women was 86.96 and for the men 84.72. This difference in favor of the women thus correlates very closely with the difference shown in this series of tests. We may, therefore, conclude that the women did better in this series of tests because they were a better selected group.

Whatever may be the explanation of this apparent sex difference the fact that such a difference occurs serves to make conspicuous *the opposite results obtained in the logical relation tests. Here the rate of efficiency is in favor of the men and against the women.* This difference is made apparent by the two charts of curves.

In each of the group I tests the women use less time; in each of the logical relation tests they use more time. The reversal of superiority is so complete and so striking as to deserve consideration.

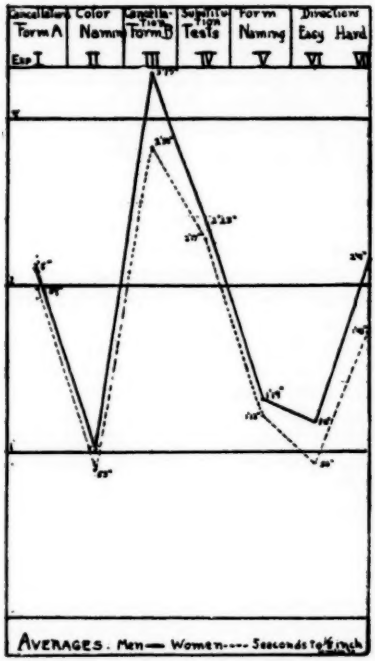

Figure I. Solid line, average of 16 men; broken line, average of 12 women. Distance on ordinates indicate time in seconds occupied in completing entire task. *Women excel men in each test.*

The factors which influence the rate of association are so numerous that variations in reaction rate are to be expected. These influences are found in these experiments but it is difficult to see why any one of them or all of them together would so operate as to produce the exact reversal of sex differences that appear. Thus a possible cause of the reversal

might be the method of experimental procedure. This, however, was standardized for each test at the beginning and remained the same for that series throughout. Men and women were treated alike. The position at the table, the

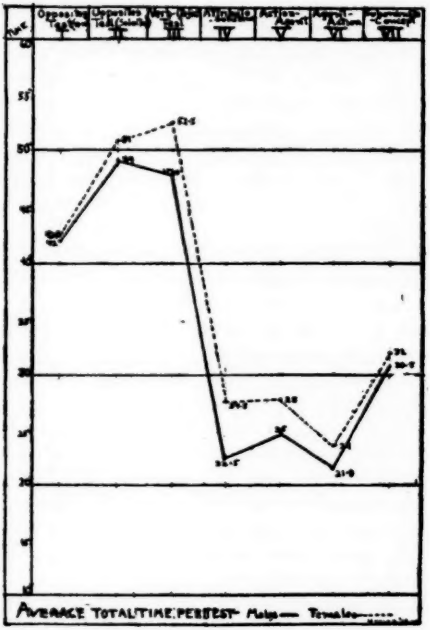

Figure II. Solid line, average of 16 men; broken line, average of 12 women. Distance on ordinates indicates time in seconds occupied in completing entire task. *Men excel women in each test.*

instructions at the beginning, the manner of presenting the stimulus, the recording of time remained the same for all subjects in a given test.

A second cause might have been the unfamiliarity of material. The subjects were mostly college juniors, seniors, and graduates. The simple material used in the tests must have had practically the same familiarity to all. This may

have been slightly less true of the logical relations tests. Yet even here the associations called for are all so commonplace that the occasional unfamiliarity reported by one or more subjects could not have greatly altered the total results.

A third possible cause might have been the attitude of the subjects toward the experiment. It might have been that the women were more interested in the Group I tests and the men more alert in the logical relations test. There does not, however, seem any valid ground for assuming this in these tests. All the subjects were seriously minded in the experiments and understood from the beginning that it was important to do the work rapidly. While there may have been individual variations from test to test it seems quite improbable that either sex should have a change of attitude sufficient to account for the changed results.

A fourth possibility is that the personality of the experimenter might have affected the subjects differently and thus altered the rate of efficiency. It is to be noted, however, that in the Group I tests there were seven experimenters all producing the same results, and in the logical relations test there were two experimenters, one man and one woman, both securing like results and results diametrically opposed to the results of the first seven. In fact the reverse opposites test was devised and given to a woman to make the tests with the aim to see if the experimenter in the logical relations tests was the cause of the reversal. The results from this test, however, were in harmony with the results of Dr. Kempf. It, therefore, seems safer to look elsewhere for the cause of the difference.

If one turns from these general conditions, none of which seems adequate as an explanation of the differing results obtained from the two sets of tests, to an analysis of the results of the logical relations tests he finds as one outstanding fact the presence of confusion as evidenced by lengthened reaction times. This lengthening of reaction was frequently several times the normal average reaction. Thus one subject whose normal time was one second consumed 5.8″ in responding to the word *win*. In such a case one reaction was practically the equivalent of six and it became necessary in computing the normal average to devise a method for eliminating such unusual reactions. For such elimination the following rough method was adopted. First the average of the series was found. To this average was added twice the average varia-

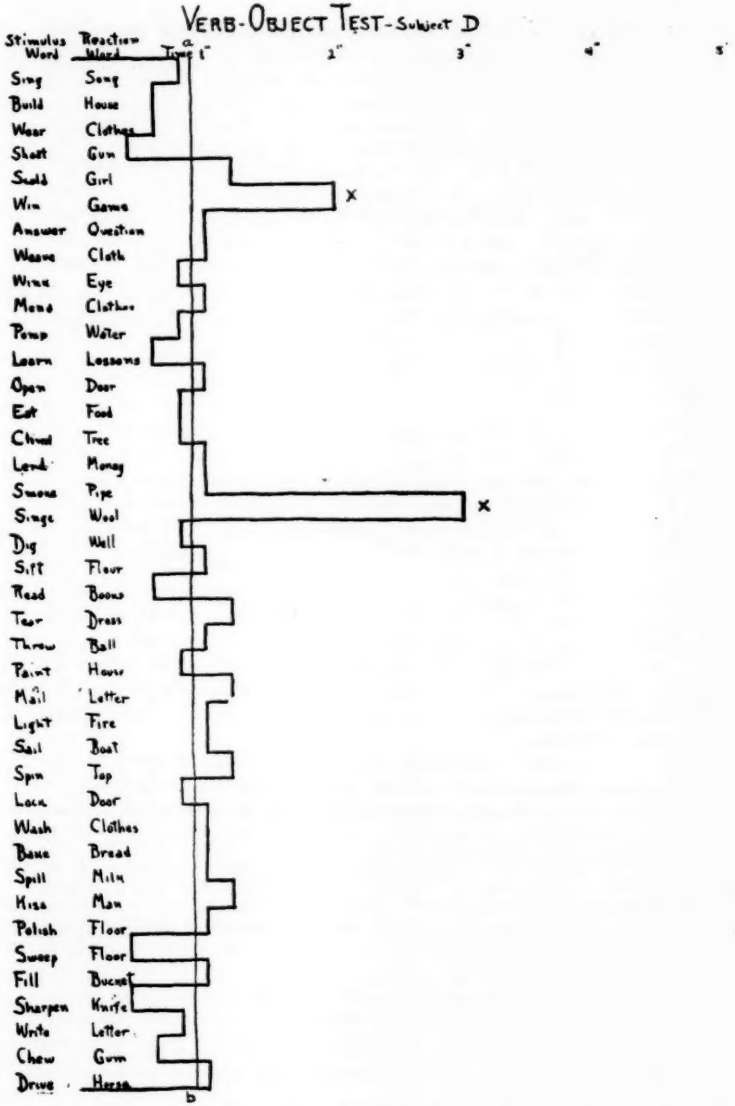

Figure III. A page showing method of recording reactions and the process of eliminating abnormal reactions. The line ab marks the average. The two reactions marked X were eliminated in making the second average which is the basis of the curve in figure II.

tion as determined by estimation.[2] All reactions exceeding this time were eliminated as abnormal. For the others a new average was found which was counted as the normal average for the individual in question. This average which is the basis of the curve on page 417 does not, therefore, involve those reactions which were highly costly in time.

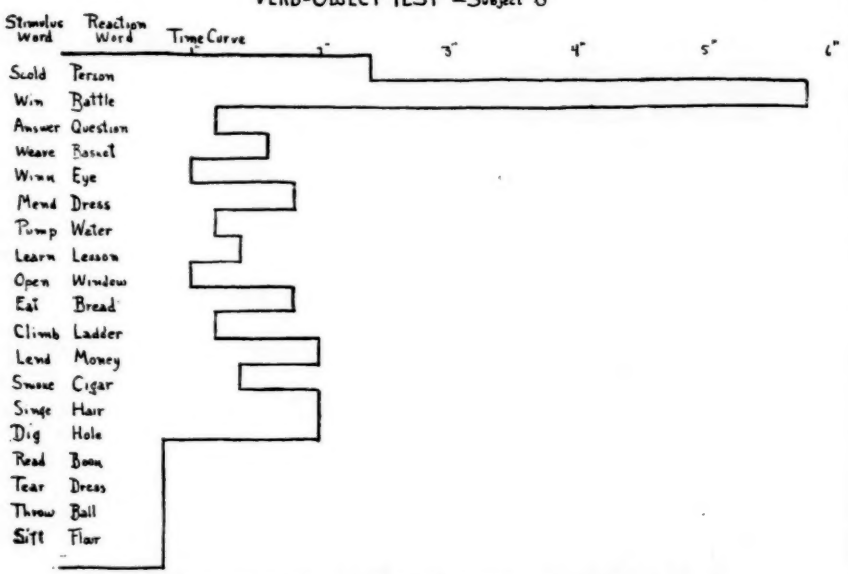

Figure IV. Verb-object test showing conflict, repression, substitution, overlapping or confusion, confession and return to normal reactions. The word *win* brought up a visual image of a valentine with a verse about "winning heart." The subject did not wish to say "heart" because it "sounded silly." "Heart" was therefore suppressed and "battle" was substituted. The effect of this confusion continued to some extent until the subject voluntarily "confessed" the foregoing substitution. The last four reactions illustrate an effectual readjustment.

[2] The records were taken on graphing paper. The length of reaction time was indicated by horizontal lines as shown in figure III, page 419. In eliminating the excessively long reactions a line representing the average time of all reactions was drawn from top to bottom of the chart. The experimenter then estimated the average

The elimination of the abnormal reaction, however, does not in certain cases eliminate the *influence of that particular reaction.* In the case of the word *win,* noted above, the lengthened reaction time is found to be due to a case of conflict followed by suppression and substitution. The subject, who was a young woman, explained the experience as follows: "The word win recalled a visual image of a valentine with a verse about winning heart. Heart sounded silly so I repressed it and substituted 'battle.'" This explanation, however, was not given until sometime after the reaction. Twelve other reactions intervened and the effect of suppression is shown in the disturbed reaction times which follow the conflict. After the twelfth reaction the subject volunteered the explanation given above. The explanation acted as a catharsis to relieve the conflict and a normal adjustment followed as appears from the reaction times of the associations which follow the explanation.

It is evident then that the elimination of the one exceedingly long reaction time does not eliminate the effect of conflict arising in the case of the particular reaction eliminated. A considerable number of the succeeding reaction-times were lengthened with the result that the average time for the whole series is greater than it would have been if the conflict had not arisen. A fair inference from this and similar cases where conflict occurred is that the presence of conflict tends to lengthen not only the total time of a series of reactions but also the average time of the individual reactions exclusive of the particular conflicting reaction in question.

It does not yet appear, however, why this phenomenon of lengthened reaction time due to conflict should make a distinct sex difference. If we turn to the number of conflicts which the two sexes show a reason is apparent. The number of conflicts for the women is greater than for the men in every test but two. For the seven tests the number of conflicts stand in the following ratio:

	Men	Women
Reverse opposites test	3.75	5.
Opposites test	6.25	8.33
Verb-object test	6.6	10.
Attribute substance test	1.2	6.6
Action-agent test	1.2	5.
Agent-action test	3.1	1.6
Subordinate-concept test	6.6	2.5

variation from this line and all reactions that were more than twice this variation were eliminated as abnormal. This method while crude is speedy and served to get rid of the widest variations. It is at the same time more accurate than direct judgment.

It will thus be seen that if the presence of conflict causes a lengthening of reaction time this would account for the women having a longer reaction time than the men in all but the last two tests.

Inasmuch, however, as these last two tests have fewer conflicts for the women than for the men and yet the times for the women are longer a further explanation is needed. This further explanation is found in the presence of less costly conflicts. It must not be supposed that when the long reaction times have been eliminated we have thereby gotten gotten rid of all the cases of conflict. It is perfectly clear from the records of individuals that there were numerous conflicts which did not come under our scheme of elimination. To discover these the following method was used. If a range of fluctuation covering one second is allowed between the lowest and the highest reaction times, it is found that the women tend to transgress this limit oftener than do the men. Thus in the verb-object test the twelve women transgressed the limits of reaction either below or above, almost always above, 64 times while the sixteen men exceeded the one second range only fifty times. This gives a ratio of 5.7 for the women to 3.1 for the men. The ratio of conflicts computed in this way for the other tests is as follows:

	Women	Men
Opposites test	3.5	1.8
Reverse opposites test	2.8	1.3
Subordinate concept test	3.1	1.9
Action-agent test	2.1	1.5
Agent-action test	1.8	.8
Attribute-substance test	1.7	1.2

The ratio for whole group of tests is 21 for the women to 11.6 for the men. Thus it is seen in every one of the logical relations tests the ratio of fluctuations is considerably greater for the women than for the men.

It thus appears that the reason the women fall behind in the logical relations tests is that they were more subject to confusions which produce wide fluctuations in reaction time, the fluctuation being usually in the direction of lengthening the time.

The data at hand do not, however, enable us to judge surely as to the cause of the confusions. It is not probable that it was of the interference type due to overlapping stimulation. This sort of confusion occurred in the tests of Group I especially in the naming tests and there is no reason to

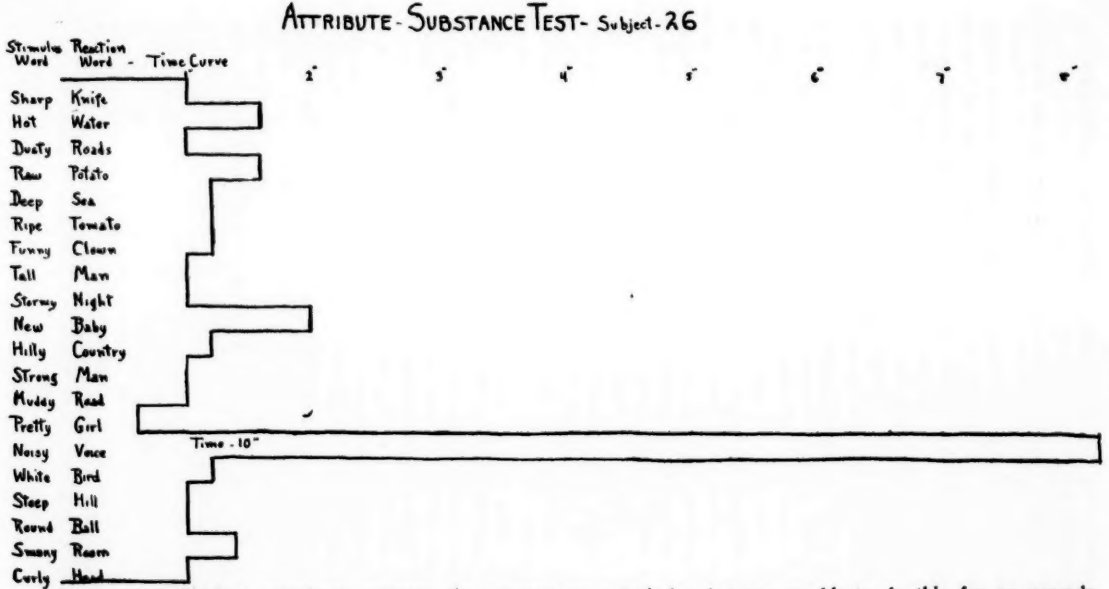

Figure V. Subject sought to suppress the spontaneous association but was unable to do this for 10 seconds when he laughed at his "ridiculous plight." He explained that the word "noisy" brought up a whole constellation of ideas, images, etc., connected with an incident of the previous day. His effort at suppression being unsuccessful, he laughed and the sound of his voice suggested the word "voice" which he spoke.

believe that there is any ground in this sort of confusion for sex differences.

In some of the cases of confusion where we have introspective data the lengthening of reaction is due to the suppression of spontaneous associations and the substitution of secondary associations. The instances in which we have these introspective data are not numerous but they are definite and occur among both sexes. Thus a young man responded "'girl" to the word "pretty" in .6 seconds. To the next word "noisy" he failed to respond for ten seconds and then began to laugh. The sound of his own voice suggested the word "voice" which he spoke. He explained his hesitation as follows: on a previous day he had been giving an association test and used the word "kiss." The subject, who was a young woman, responded "noisy." This had amused him and when the experimenter in this test said "noisy" it brought back the whole constellation of ideas, images, etc., connected with the previous experience. He was unable to suppress the matter entirely and could make no substitution until he laughed at his own "ridiculous plight." This broke the cramped set of mind and he found a word.

If we could infer from such cases that all the confusions were due to conflict, suppression, and substitution we should have to explain why such conflicts were more numerous among the women than among the men. Here are a group of twelve persons whose associative mechanisms in one set of tests work with greater efficiency than those of another group of sixteen persons but who in a second set of tests find their association processes blocked, confused, diverted and inefficient. Analysis indicates that the blocking is due to inhibitions accompanied by emotional excitement. The inhibitions in turn seem due to the tendency of the individual to protect himself from embarrassment. The mind exercises a censorship over its overt expressions suppressing those felt to be inappropriate to the situation and selecting others. This process consumes time with the result that those persons in whom the tendency is strongest occupy the longer time and thus appear by the measure of the tests less efficient than those in whom the tendency is weak.[3]

[3] It should be pointed out that the tendency to be "on guard" would not only influence the length of time in the actual case of conflict but just because the tendency was there it would tend to lengthen the reaction time even though no conflict arose. A mind on guard against self-surprise and self-embarrassment is perforce a slower mind than one from which this tendency is absent or at a minimum.

To reason in the opposite direction, the women, inasmuch as they show the shortest average association time in those tests where no conflicts are possible and the longest average reaction time in those tests where conflict is likely to be frequent must possess the tendency toward suppression to a greater degree than do the men.

Our data, while allowing this as a probable inference, hardly warrant us in asserting it as a proved fact. There are some cases of lengthened reaction where there is no apparent effort to suppress. Thus one person responded "potato" to the word "raw" and remarked that he was fond of potatoes. To the word "new" he said "baby" and noted a recent event in his friend's family. To each of these associations there was a slight emotional tinge which might very well account for the delay of response. Such delays, however, are not likely to be so expensive in time as the cases of suppression and substitution.

Again it might be that differences in imagery might cause differences in reaction time, concrete imagery being more expensive than verbal. This might very well account for individual differences and if it could be shown that the women had more concrete imagery than the men this might have a bearing on the sex differences.

We happen to have the imagery studies made in the course of the class-work. These are not thoroughly accurate, but so far as they go they do not seem to show any sex differences in imagery such as would account for the differences shown in the tests.

However, the conclusions that the differences shown in these tests are due to the greater tendency of the women to be "on guard" against embarrassment can be regarded as tentative only. The fact if it can be shown to be a fact of general application is so important that further investigation along this line should be made. It must be said that the experimental study of adult sex differences has not as yet yielded any very significant results. There seems to be a general conviction that there is a feminine type of mind different from the masculine but in just what this difference consists we have as yet no measured experiments to prove. In view of this lack of scientific data on the matter it seems worth while to set forth the facts and suggestions contained in this paper.

IMPROVEMENT IN A PRACTICE EXPERIMENT UNDER SCHOOL CONDITIONS

By M. E. Donovan and Edward L. Thorndike, Teachers College, Columbia University

The importance of knowledge concerning the amount and rate of improvement due to practice under school conditions and concerning the effect of equal amounts of training upon the individual differences found amongst a given group in a given trait is so great that we venture to report a very slight contribution to it.

The experiment consisted in measuring the effect of approximately sixty minutes' practice (in approximately 30 periods of 2 minutes each, given twice daily for the five school days of the week) at adding columns, each of ten digits (0 and 1 not being used). The subjects were twenty-nine boys in a fourth-grade class in New York City.

The score used as a measure of efficiency was the number of examples done *correctly*. That is, no credit was given for an example containing any error. We are unable to report how much interest in the work and in improvement there was.

The group as a whole improved, as the result of the hour's practice, from an average score of 2¾ examples correctly done per minute in the first two periods, to a score of 4½ examples done correctly per minute in the last period. The results thus emphasize the very great gain probably to be expected from applying the method of the practice experiment to certain functions whose improvement is a part of the school curriculum. The individual amounts of improvement are shown in Table I.

If we compare the improvement of the eight boys who showed the least ability at the start (4, 4, 5, 6, 7, 7, 8 and 8 examples done correctly in four minutes) with the seven who showed the most ability at the start (21, 19, 16, 16, 15, 14 and 14 examples done correctly in four minutes,) we find that the latter made equal or greater gross gains (8 on the average to 7.6 for the less able group). What happens when individuals of different abilities are given equal practice in addition is shown still more clearly by Table I, which gives the average scores for: First, the four boys of initial ability 4, 5 or 6; Second, the four boys of initial ability 7 or 8; third, the four boys of initial ability 9; fourth, for the seven boys of initial ability 10, 11 or 12; fifth, for the three boys of initial ability 13; sixth, for the five boys of initial ability 14, 15 or 16; seventh, for the two boys of initial ability 19 and 21.

These results, showing so little power of equal additions to training to reduce individual differences, make it improbable that a very large fraction of the differences found among school children can be justly attributed to differences in amount of training. Since the argument on this point has been stated by Thorndike (*Amer. Jour. Psychol.*, xix, 1908, 383 f.), and by Wells (*Amer. Jour. Psychol.*, xxiii, 1912, 75-88), we will say no more about it. The results of the present study are in entire accord with the view presented by these authors.

TABLE I

SUCCESSIVE SCORES IN ADDITION: FOURTH-GRADE PUPILS

Individual	Number of periods practiced	First two	Second two	Third two	Fourth two	Fifth two	Sixth two	Seventh two	Eighth two	Ninth two	Tenth two	Eleventh two	Twelfth two	Thirteenth two	Fourteenth two	Fifteenth two	Sixteenth two	Gross gain,—First two to Last two
x	25	4	6	5	8	10	9	9	4	3	4	9	6	4				0
y	31	4	7	7	9	5	8	8	9	8	13	11	9	9	12	10	11	7
r	32	5	4	5	5	4	4	5	5	6	8	5	6	7	9	10	11	6
f	31	6	6	10	12	7	11	9	14	17	14	13	12	9	16	13	14	8
Average for x, y, r and f		**4.8**	**5.8**	**6.8**	**8.5**	**6.5**	**8**	**7.8**	**8**	**8.5**	**9.8**	**9.5**	**8.3**	**7.3**	**?**	**?**	**?**	**5.3**
s	32	7	13	15	16	16	19	13	16	21	17	19	21	15	20	19	22	15
B	31	7	7	6	8	7	9	10	10	5	10	9	7	9	11	10	11	4
m	32	8	10	13	13	13	12	12	13	10	12	14	14	14	14	16	15	7
v	32	8	10	10	15	11	16	11	14	16	20	20	16	23	16	18	22	14
Average for s, B, m and v		**7.5**	**10**	**11**	**13**	**11.8**	**14**	**11.5**	**13.3**	**13**	**14.8**	**15.5**	**14.5**	**15.3**	**15.3**	**15.8**	**17.5**	**10**
e	31	9	6	8	12	9	10	14	9	8	11	13	14	11	16	17	16	7
k	32	9	8	4	9	8	7	10	8	6	7	9	6	7	11	9	9	0
n	29	9	10	12	12	12	10	9	12	11	13	15	15	14	13	16		7
A	32	9	14	13	14	15	12	16	15	19	18	17	17	21	21	21	16	7
Average for e, k, n and A		**9**	**9.5**	**9.3**	**11.8**	**11**	**9.8**	**12.3**	**11**	**11**	**12.3**	**13.5**	**13**	**13.3**	**15.3**	**15.8**	**14** (app.)	**5.3**

TABLE I—*Continued*

Individuals	Number of periods practiced	First two	Second two	Third two	Fourth two	Fifth two	Sixth two	Seventh two	Eighth two	Ninth two	Tenth two	Eleventh two	Twelfth two	Thirteenth two	Fourteenth two	Fifteenth two	Sixteenth two	Gross gain,—First two to Last two
b	27	10	11	16	15	10	13	16	14	18	19	19	17	17	20			10
w	30	10	12	12	13	11	17	18	11	11	15	14	12	14	12	13		3
i	32	11	13	12	13	12	15	15	12	10	15	13	14	16	18	18	19	8
C	31	11	13	16	17	11	14	12	14	17	12	12	19	19	14	9	13	2
c	31	12	14	11	13	10	11	15	9	14	16	15	11	11	14	12	18	6
j	32	12	14	15	14	17	21	15	13	18	18	16	21	18	21	22	25	13
q	32	12	10	14	13	13	12	13	15	15	11	12	14	11	10	11	16	4
Average for b, w, i, C, c, j and q		11.1	12.4	13.7	14	12	14.7	14.9	12.6	14.7	15.1	14.4	15.4	14.9	15.6	14.7 (app.)	17.4 (app.)	6.6
t	32	13	10	10	13	14	16	11	15	10	9	14	13	14	15	15	13	0
u	32	13	10	16	17	14	16	19	18	20	19	20	20	21	27	23	25	12
z	32	13	11	18	13	17	13	19	21	15	19	16	20	20	17	25	26	13
Average for t, u and z		13	10.3	14.7	14.3	15	15	16.3	18	15	15.7	16.7	17.7	18.3	19.7	21	21.3	8.3
a	32	14	12	16	16	14	15	17	13	16	16	15	19	19	22	20	16	2
d	29	14	13	10	12	12	12	16	12	7	12	9	14	14	10	16		2
p	29	15	9	15	18	16	13	12	11	17	13	14	12	15	15	16		1
h	32	16	15	18	25	23	23	25	26	28	28	32	29	25	31	34	35	19
o	30	16	16	19	24	24	25	24	21	22	21	15	23	25	25	26		10
Average for a, d, p, h and o		15	13	15.6	19	17.8	17.6	18.8	16.6	18	18	17	19.4	19.6	20.6	22.4	?	6.8
g	28	19	21	20	23	21	24	22	26	27	29	31	32	32	26			7
l	32	21	24	29	27	27	27	25	31	26	25	28	29	28	28	30	36	15
Average of g and l		20	22½	24½	25	24	25½	23½	28½	26½	27	29½	30½	30	27	?	?	11

DISCUSSION

THE METHOD OF EXAMINATION

By E. B. Titchener

During the past few years we have published, from the Cornell Laboratory, a number of studies in which we sought to test the 'method of examination' or 'method of questions.' We owe the suggestion of this work, for the most part, to the publications of the Würzburg school. It is clear that the method, if it is reliable, greatly extends the scope of experimental psychology; but it is also clear that, if the results obtained by Külpe and his co-workers are accepted, the system of psychology must be rebuilt from the ground up. We were ourselves unable, when we repeated their experiments, to confirm some of the most important theses of the Würzburg investigators; we thought that we found in their method a definite and familiar source of error; and we therefore saw no reason for an immediate rewriting of our psychology. It seemed best to suspend judgment until we had made trial of the method in our own behalf.

No laboratory, of course, can devote itself *in perpetuum* to a single method and a single range of topics. We have, I hope, given the method of examination a fair test; and we may now, I believe, summarise—in a tentative and provisional way—the conclusions that we have reached. That is the object of the present paper. And since a scientific standpoint is most sharply defined by contrast, I shall take account, in what follows, of the—uniformly unfavorable—criticisms which Dr. Koffka, of Giessen, has published in the *Zeitschrift für Psychologie* upon the studies to which I have referred. In matters of detail, some of Dr. Koffka's objections seem to me to be well taken, others appear positively to miss the point; I shall, however, try to subordinate minor differences to the main questions at issue. I have, on my side, this principal charge to bring against our critic: that he does not recognise the serial nature of our work; he does not see that we have been feeling our way to clearness, step by step and problem by problem; he does not realise that the progress from Okabe's article on *Belief* to my own discussion of *Description and Statement of Meaning* is as real—*si parva licet componere magnis*—as the progress from Marbe to Bühler. Otherwise he would surely have noted that the shortcoming of an earlier paper is redeemed, at least so far as intention and effort are concerned, by the plan of some later study.

Another introductory remark must be made. It is natural that a student, who has spent two or three years upon this method of examination—reading, criticising, observing, planning, interpreting—should, when he comes to publish, lay stress upon the positive outcome of his work. He has been no less patient, no less ingenious, no less single-minded than his colleague, whose study of sensation or perception has been based upon methods of a stricter type; and he is eager to show that his labor has not been wasted; he wishes to make his contribution to the store of psychological fact. It is,

nevertheless, not quite fair to take him at his word, and to judge his results as they are offered. The reader with a wider perspective will understand that the general method is itself on trial, and that, while the statements of the author's *Summary* are a proper subject of criticism, yet the critic fails of insight if he forgets their genesis, their attainment by a practically unaided observation. We shall never discover, by the naked eye, the details that lie plain in the field of the microscope; but where the microscope cannot be employed, a gross description may have real scientific value. We shall go astray if we bring the eye into comparison or rivalry with the microscope; but the critic is also at fault when he confuses the two orders of observation, when he judges them both by the same standard; for results, after all, are a function of method.

Okabe on Belief.[1]—In his study of belief, Okabe had six observers, four trained and two relatively untrained.[2] His method was to lay before these observers, in visual or auditory form, sentences or mathematical expressions which were calculated to arouse belief or disbelief; or, later in the work, to present sentences or mathematical expressions in pairs, the one member of which should arouse belief, the other disbelief.[3] The observers were to attend and to understand; if then belief or disbelief appeared, they were to close their eyes and to dictate a report of their experience. No time-limits were set, and no time-records were taken.[4]

The main results of the study were three. The first was that the experience of belief may be either explicit or implicit; that is to say, belief may appear as a complex course of specific content-processes, or may be bound up with, incorporated in, a particular consciousness, with no conscious representation beyond the mode of occurrence of this consciousness itself. The distinction thus drawn is no new psychological discovery; the reports of Okabe's observers confirm and extend observations of Ach, Messer and Störring;[5] but the independent confirmation, and the extension to the untrodden field of belief, are surely worth while. The second result was that belief and disbelief are consciousnesses of the same kind. This, again, is not a new discovery. "The true opposites of belief, psy-

[1] This JOURNAL, xxi., 1910, 563-596.

[2] Koffka remarks that the reports of the untrained observers, which "differed in important points from those of the others, were dismissed as *Kundgabe*" (*Zeits.*, lxiii., 1912, 398). The facts in the case will be found in Okabe's paper, 575 f.

[3] Okabe speaks here of "a method [not *the* method] of paired comparisons" (580). I do not know why he should not; but Koffka objects to the name (*Zeits.*, 397). Okabe gives (*loc. cit.*) the reasons for his change of method.

[4] Koffka thinks that the duration of the single test, and the number of tests made at a sitting, should both have been recorded (*Zeits.*, 397). The criticism is just. We supposed, at the time, that we might obtain more complete descriptions if the observer knew that he was *not* to be timed (*cf.* Wundt's criticism of 1907, *Kleine Schriften*, ii., 1911, 277, 281, 293); but later experience showed that observers very soon grow accustomed to the stop-watch. The sittings were always of an hour's length, and our papers show the number of tests made in every hour; we did not find that this number was significant; but the fact should have been stated.

[5] *Op. cit.*, 592 f.

chologically considered," says James, "are doubt and inquiry, not disbelief;" and Bain had said the same thing before him.[6] Okabe thus confirms, by the evidence of his observers, an opinion already expressed as a personal conviction by psychologists of note. The third result—which is, in fact, a set of results—consisted in a rough analysis and differentiation of the experience of belief as it varies with type, affective disposition, and so forth. Here is new material; and its value is the value that attaches to a first, direct, observational report of a fairly complex situation.

Koffka, however, has two sweeping objections. In the first place, Okabe's trained observers show theoretical bias, and his own conclusions rest upon an arbitrary selection of reports.[7] I can hardly be expected to meet this criticism otherwise than by referring the reader to the article itself. In the second place, we have not attempted a real analysis: we do not say whether all our observers attached the same meaning to 'belief' and 'disbelief;' we do not say what visual quality (*Sehqualität*) is involved when an observer 'sees' agreement or disagreement; and we do not undertake a special study of the *Aufgabe*.[8]

As regards the *Aufgabe*—I take up Koffka's points in reverse order—I grant that we leaned somewhat heavily on the work of our predecessors in Germany; we sought, later, to correct this mistake. As regards the 'seeing' of agreement, I can only suppose that our critic has, in some extraordinary fashion, misread Okabe's statements. In his first analytical summary of the reports obtained from one of his observers, Okabe speaks of "a 'perception' of agreement or disagreement, of harmony or disharmony, which was difficult to analyse, but seemed in every case to be preponderantly visual." So far is he from reference to a 'visual quality' of agreement that he hesitates to use even so loose a term as 'perception.' Later on, when the reports grow fuller, he allows himself to speak outright of the perception of agreement; and finally, when he summarises the whole body of results for purposes of confrontation, he says: "the core of belief-disbelief is to be sought in the arrangement and behavior of visual images."[9] Is it necessary to point out that arrangement and behavior are spatial and temporal affairs, independent of sensible quality? or that there is a progressive analysis as this observer gains in practice?

There remains the objection: "nirgends ist die Frage aufgeworfen, ob alle Vpn. dasselbe unter belief und disbelief verstanden." The critic is mistaken: there is a passage in which that question is explicitly raised.[10] I do not rest, however, in this formal reply. We find, in the reports, cases of a passive acceptance which follows upon understanding; we find typical consciousnesses which we and the observers call belief and disbelief; and we find consciousnesses of certainty, of positive conviction. We find that there are various shades or *nuances* of belief, which the observers seek to express by adjectives. All these things are set down, in such detail as the method permits. And our object was, of course, to learn what sorts of consciousness are covered by the term belief, as this term

[6] *Ibid.*, 564 f.
[7] *Zeits.*, 397 f.
[8] *Ibid.*, 398 f.
[9] *Op. cit.*, 570, 577, 583, 589, 592.
[10] *Ibid.*, 576.

is ordinarily employed,—not to erect a norm of belief, to which the observers should be brought to conform.

Clarke on Conscious Attitudes.[11]—Clarke's aim is to bring the conscious attitudes—"certain large and comprehensive experiences, not evidently imaginal in character"—to the test of introspective observation, and thus to discover whether or not they are analysable. Her early tests yield a number of incidental analyses, of which she writes: "Many of these incidental analyses are, evidently, imperfect. Even at the best, the observers might report in large and sweeping phrases, such as 'bodily attitude' or 'organic sensation.' There is, however, no doubt that the reports were intended at the time to represent the attitudes themselves, and not merely incidental or concomitant occurrences." And she supports this position by the direct acknowledgments of her observers.

"It is obvious," says Koffka, "that analysis meant, for the author and her observers, nothing else than the exhibition of the sensory contents present at any given moment. . . . These sensory contents may be irrelevant to the thought, or may be the necessary condition of the arousal of a thought, or may finally be the thought itself. Why this third possibility should alone be recognised, neither the author nor her observers explain."[12] The reply is that this possibility was not alone recognised. It was precisely because there were other possibilities that the observers were instructed, at the outset, to give complete introspections, and were confronted, later on, "with an outline of their reports upon various attitudes, and were asked to say whether, so far as they could remember, the analyses were, as analyses, correct. The regular answer was that they were correct, and in several cases the observer added, of his own accord, that he could reproduce the attitude, at the moment, and that it corresponded with the analysis given." The sensory contents were, then, certainly not irrelevant; nor were they, so far as the observers could tell, the condition of some further and specific conscious contents; they were the given conscious factors of the attitudes themselves.

But, the critic goes on, "it is sheer enigma that an observer can say, by direct observation, that 'approval' is 'pleasantness with some general kinaesthesis.' Pleasantness and general kinaesthesis are first of all just that,—pleasantness and general kinaesthesis."[13] To be sure they are, when you treat them in separate chapters of a text-book. But the objection, taken literally, denies the possibility of psychological analysis in any field. What we have in the present case is simply this: that under the instructions given, and under the conditions of the experiment, the 'attitude of approval' factorises into pleasantness and some general kinaesthesis. Nobody asserts that the analysis is adequate; Clarke herself, as we have seen, regards her incidental analyses as incomplete; the point is that the attitude, in the particular circumstances, does not wholly resist analysis, but factorises in the manner stated. Clarke then proceeds to more detailed analyses, which the reviewer dismisses as worthless.

But, once more, Clarke speaks of imagery as "carrying thought. Yet in that case thought would be, after all, a specific conscious

[11] This JOURNAL, xxii., 1911, 214-249.
[12] *Zeits.*, lxiii., 1912, 219.
[13] *Ibid.*, 219.

contents; and this, again, is directly denied."[14] Can the criticism be seriously intended? It would mean that every psychological term must find representation as a *besonderer Bewusstseinsinhalt*. It would mean that when, for instance, we have analysed an 'emotion,' we shall have to add to our analytical results a specific conscious contents 'emotion.' And more than that: it would mean that every general term in logic or ethics or aesthetics must have its special conscious representative: whatever psychological analysis may discover in the judging, approving, appreciating consciousness, there must always be a specific contents of judgment, of approbation, of appreciation! For it is clear from the context that the thought which Clarke's imagery 'carries' is understood by her to be logical thought.

I can only suppose that the reading of Clarke's paper made upon the reviewer a generally unfavorable impression, and that, when he came to write his review, he was more concerned to express dissatisfaction than to work out psychological principles. At all events, I cannot see that a system of psychology would be possible, if these principles are to stand. I hear the notes *c-e-g* struck on the piano. I may recognise the chord, as the common chord in the key of *c*-major. I may also analyse it into tones and noise; I find, perhaps, a form of combination, an elementary aesthetic feeling, an organic reverberation. Is it "sheer enigma" that a practised observer can resolve 'the common chord in the key of *c*-major' into these components? and will Koffka deny the possibility of psychological analysis at large? Or again: I say that my discrimination, in certain terms of a psychophysical series, was based upon absolute impression. Would discrimination, in such a case, "be, after all, a specific conscious contents," although "this is directly denied?" Koffka must, apparently, reply in the affirmative. I submit, once more, that the psychological principles which underly his criticism are at least unfamiliar, and that a mere appeal to them as axiomatic does not justify his adverse judgment of Clarke's work.

The remainder of Clarke's article, which is dismissed as beneath consideration, is taken up with a demonstration of graduated steps from clear imagery to 'imageless thought,' and with detailed analyses, explicit and genetic, of particular conscious attitudes. Clarke, I may add, discusses the *Aufgabe;* registers the times of reaction; and often mentions the place of a report in a sitting or series.[15]

Titchener on the Psychological Self.[16]—I should not have been surprised if our critic had ignored my note on the *Consciousness of Self*. As it is, however, I must defend myself against a hailstorm of critical condemnation. I had found, in a current text-book, the statement that 'I am always, inattentively or attentively, conscious of myself, whatever the other objects of my consciousness.' This statement was at variance with my own experience, and I wished to put it to a preliminary test. Koffka objects, first, that the concept of self, "which springs from a whole number of sources, but assuredly not

[14] *Loc. cit.*

[15] Okabe published in October 1910, Clarke in April 1911. Koffka's reviews were apparently sent to the editor at the same time: that of Clarke came out in November, that of Okabe in December 1912. May I not justly express surprise at Koffka's failure to point out that some of the glaring defects of Okabe's method had been corrected by Clarke?

[16] This JOURNAL, xxii., 1911, 540-552.

from those of a scientific psychology, is employed simply as a psychological concept."[17] That is true. It was the psychological self that I was concerned with, the self that is discussed in the psychologies as part of the subject-matter of psychology; all other selves were indifferent to me. If I have erred, a great many others are also at fault. Should not the blame be shared? and should we not be told why a psychologist may not investigate the conscious representation of a concept—whatever its origin? Koffka, to be sure, adds that "wir die wichtigsten Aufschlüsse wohl von der Pathologie zu erwarten haben." Let that be granted: and I, at any rate, have nowhere denied it: still, is it, in the present connection, anything but a paralogism? For the generalisation that I was combating is certainly not derived from pathological sources.

"It is simply assumed," the critic proceeds, "that everybody understands by the word self-consciousness not only a perfectly definite experience, but also an experience that is somehow the same (*irgendwie gleichartig*)." The objection, if it can be urged with any show of reason at all, affects the statement that I was examining rather than my own procedure. For my second question called for a description of the self-consciousness, which should be made "as definite as possible. Is the consciousness of self explicit (*e.g.*, visual image, organic sensations)," I asked, "or implicit (intrinsic to the nature of consciousness, inherent in the course of consciousness)? Can you bring out the character of the self-consciousness by comparing or contrasting it with other phases of a total consciousness?" Neither in the formulation of this question nor in my treatment of the reports did I make the assumption attributed to me.[18]

"As we have to do in the present instance with an experience of extremely complicated structure," the reviewer continues, "this [uniformity] is not self-evident, but on the contrary is highly improbable." I can, again, only wonder at my critic's psychological principles. Genetically, the concept of the self may be of an extreme complexity; but is there any reason *a priori* why, in the consciousness of the educated adult to-day, it should show an "extremely complicated structure?"

There remain two objections of a more practical kind. "There are," observes our critic, "far simpler experiences in face of which the author's method meets with flat failure." I myself refer to cases in which a somewhat similar method proved unreliable, and mention the additional safeguards upon which I rely.[19] Koffka, however, is speaking of sources of error other than those that I had discovered. He must, therefore, have in mind cases in which my method was exactly—and fruitlessly—anticipated. I do not know of them; and he, unfortunately, does not give references. He objects, finally, that the method "introduces into the laboratory the old psychology of reflection."[20] I should say that the method furnishes a rough way of discovering whether the old psychology of reflection is already

[17] *Zeits.*, lxiii., 1912, 212.
[18] *Op. cit.*, 545 f., 550 f.
[19] *Ibid.*, 540.
[20] It is characteristic of the criticism evoked by this method of questions that the charge which Koffka here brings against me, in particular, is precisely the charge brought by Wundt against the Würzburg school in general: see *Physiol. Psychol.*, iii., 1911, 551 ff., esp. 553 f.

entrenched in the laboratory; I do not see how a request for direct observation, with a judgment of presence or absence, could of itself introduce that psychology. I return to this point later.[21]

Jacobson on Meaning and Understanding.[22]—The novel feature of Jacobson's work on the perception of letters and the understanding of words and sentences is the attempted separation of 'description' from 'statement of meaning.' The observers were required "to put direct description of conscious processes outside of parentheses, and statements concerning meanings, objects, stimuli and physiological occurrences inside."[23] Jacobson, now, is taken to task, first of all, because he further instructed his observers to report their experiences in strict temporal order. "It has not occurred to the author that he could do better by setting various temporal limits, objectively, to the process under examination."[24] This is a little harsh, when even Okabe had had recourse to fractionation![25] But, indeed, the critic has not read with sufficient care. Fractionation may give you the objective temporal order of occurence; but it tells you nothing of the observer's temporal attitude during an extended report. And Jacobson was interested in the method; he wished to find out whether the time-relations noted in the extended reports of previous investigators were reliable.[26] A second point against him—that the nature of his 'statements of meaning' is not clear[27]—must be acknowledged as fair criticism. It should be said, however, if we are to continue fairly, that Jacobson was aware of this defect, and himself calls the reader's attention to it; and also that he expressly raises the question of the difference between associates which carry meaning and associates which do not.[28] Koffka charges, thirdly, that the method of reinstatement or repetition betrays ignorance of "the rules for the use of introspection."[29] As if these *Vorschriften* could be written down with mathematical accuracy, and as if a rule did not vary with variation of the conditions of observation! Jacobson refers to Wundt's article of 1907, and was familiar with the discussion of 1888.[30] He wished to learn whether, if the original conditions of the observation are restored,[31] the method of reinstatement is still

[21] P. 440 below.

[22] This JOURNAL, xxii., 1911, 553-577.

[23] *Ibid.*, 555.

[24] *Zeits.*, lxiii., 1912, 380.

[25] *Op. cit.*, 567.

[26] *Op. cit.*, 553 f. Jacobson found also that the temporal instruction "was of material aid in the correlation of 'process' and 'meaning.'"

[27] *Zeits.*, 381.

[28] *Op. cit.*, 556 f., 566 ff. A more sympathetic critic, who had observed the serial nature of our work (p. 429 above), would perhaps have anticipated a further study, dealing with this outstanding question.

[29] *Loc cit.*

[30] *Op. cit.*, 556; W. Wundt, Selbstbeobachtung und innere Wahrnehmung, *Phil. Stud.*, iv., 1888, esp. 298 f.

[31] *Ibid.*, 555. Koffka's further criticisms—as that Jacobson should have consulted the reports of tachistoscopic experiments—seem still to show that he misses the writer's interest in the general method. I am inclined to agree that Jacobson's modifications of the method of questions, ingenious as they are, do not materially advance it; but I think that they were worth trying; and at any rate there could be no question of a recourse to the tachistoscope. Again, however,

worthless. He found that his observers did not hesitate to report failures; that sometimes specific differences were noted between the two experiences; and he decides on the whole that "the results are encouraging, though we offer them only as a first contribution to the settlement of the question." These careful statements do not, surely, show "ignorance of the rules for the use of introspection!"

Titchener on Description and Statement of Meaning.[32]—In this paper I recur to some of the points which Jacobson had left obscure, and in particular to the question of the nature of his bracketed 'meanings.' I seek to characterise the attitudes implied in, or demanded by, the two modes of report which he had called for; and I find that the one is the attitude of descriptive psychology, the other that of logic or of a logical common sense. The distinction, then, is now no more difficult for us than for Bühler,[33]—though, as I need not say, it is not Bühler's distinction. And with it we seem to have gained the right to draw up that provisional and tentative summary of which I spoke at the beginning of this article,—the summary of our conclusions in regard to the method of examination. There are, it is true, many questions which still remain without answer;[34] and our own use of the method has, as the preceding paragraphs show, been roundly condemned. But the condemnation rests, in good part, upon psychological principles which are anything but secure; and I hope, in despite of it, that the following remarks may not be without value.

We are not to suppose that the method of questions has reached the limit of its usefulness. We may expect, on the contrary, that future work will extend both the range of its application and the variety of its forms. There seems, however, to be no likelihood that we shall gain from these extensions any power or any knowledge that we have not, in principle, already obtained. The method, as method, has been sufficiently tried, and may now be appraised.

I. I begin with a negative. It does not appear that the method of questions will ever avail, of itself, to settle disputed questions of a systematic kind: that it will enable us to decide, *e.g.*, for or against the distinction of 'act' and 'contents,' or to compose the issue between imageless and imaginal thought, or to prove or disprove the existence of a 'form of combination.' For, in the first place, the conduct of the method, the empirical procedure, is always open to a criticism which derives its canons from the more rigorously experimental ways of working that have established themselves in the fields of sensation and perception. Criticism of this sort will hardly influence the author of any given study; it will seem to him to be irrelevant, off the point; when he adopted the method, he will say, he took it with full acceptance of its limitations; he turned his back deliberately upon the refinements of experimentation; to pick minute flaws, to pepper the work with objections of detail, is only to do what he could do for himself; it shows a misunderstanding of his aim and intention. But then this same author, when it is his turn to criticise, finds himself in a quandary. Unless he is

it is characteristic that Koffka's recommendation is identical with that of Wundt to the Würzburg group: *Kleine Studien*, ii., 1911, 291.

[32] This JOURNAL, xxiii., 1912, 165-182.

[33] *Zeits.*, lxiii., 381.

[34] See this JOURNAL, xxiii., 1912, 507; xxiv., 1913, 154.

to confine himself to a discussion of principles, he too must come down to detail; and there his opponent will meet him with his own retort. And so the critical volleys are fired, back and forth, and neither side will acknowledge a hit. The spectator, no doubt, will form a positive opinion; yet it may be questioned whether this opinion is induced directly and solely from the results of criticism. We have, in a word, a gross and loose method which is attacked or defended with all the art and skill that an experimental methodology can provide; and we cannot hope, under these conditions, to settle the kind of question that was raised at the beginning of this paragraph. But, in the second place, the results of the method—since, as I have said, results are a function of method—must themselves be gross and loose, capable of various interpretation. So an introspective report that is published in support of a certain thesis may be seized upon by a psychologist of different persuasion, and interpreted in a radically different sense; or the charge of 'arbitrary selection' may be laid against the presentation of results, as the charge of 'bias' or 'prepossession' may be laid against the conduct of the method. For these defects I see no promise of remedy in the method itself; and in so far my experience has brought me into agreement with Wundt.

II. I believe, nevertheless, that the method is of value. I can better show wherein this value consists, however, after I have characterised the method which I think will replace the method of questions as an accredited procedure in experimental psychology. Wundt, as is well known, would base a psychology of the 'higher' processes upon a study of mental 'products,'—so that the psychology of thought would emerge, *e.g.*, from a psychological study of language:[35] a direct experimental modification of these processes is for him impossible, and their direct observation is of necessity inexact. But while one will hardly find an experimentalist who denies the importance of *Völkerpsychologie*, one will hardly find one, either, who accepts Wundt's division of the science. And the method which we must look to, if we are to transcend this division, is—it seems to me—a method of the type of Ach's 'systematic experimental introspection;' a method, that is, which secures description under experimental conditions so strictly controllable that we may hope, by manifold repetition, to attain accuracy of report.[36] We cannot, in other words, dispense, in the study of the higher processes, with the experimental aids which have helped us to an understanding of the lower.

Ach's method, however,—in the particular form that he gave it,—has been severely criticised by G. E. Müller.[37] It is dangerous. Müller

[35] See, *e.g.*, *Physiol. Psychol.*, iii., 1911, 554.

[36] N. Ach, *Ueber die Willenstätigkeit und das Denken*, 1905; *Ueber den Willensakt und das Temperament*, 1910. I say purposely "a method *of the type of* Ach's." The attempt to work out, in detail, the grounds of this qualification and the nature of the method contemplated would lead me too far afield, and would perhaps in any case be premature. It is plain, however that very many experimental procedures, over and above those employed by Ach, may be incorporated into the method. I suggest also that, in the sphere of the higher processes, direct may be supplemented by indirect modes of approach.

[37] *Zur Analyse der Gedächtnistätigkeit und des Vorstellungsverlaufes*, i., 1911, 137-143. I have discussed this criticism, from another point of view, in this JOURNAL, xxiii., 1912, 503 f.

remarks, for several reasons, to have free recourse to question and answer, aside from the fact that questioning unduly prolongs the report and thus puts too great a strain upon the memory of the observer. It is out of the question that the introspective report of a fairly complicated consciousness should be even approximately complete. The perseverative tendencies, on which Ach relies, are selective and at times misleading; and we have no proof that they are strengthened by the intent to observe. Finally, it is dangerous to suggest to the observer that the contents of the after-period are, as perseverative, identical with those of the experimental consciousness; we know of cases to the contrary; and the observer should therefore be instructed to report only such experiences as he remembers, with assurance, to have occurred during the experiment.

It is, now, a little disquieting to find that Müller's criticisms bear upon the 'systematic introspection' and not upon the 'experimental' of Ach's method; we seem to be back again in the fruitless discussions of the method of questions. But let us see! There is no physical compulsion to ask questions; the experimenter may rely upon the repetition, under identical or under modified conditions, of the task set to the observer. Or if, from lack of time or from the appearance of carelessness or of stereotyped reaction in the observer, questions seem to be indicated, then the reports that are obtained (and later reports that are possibly influenced) by questioning may be treated by themselves, and may be compared with the spontaneous reports of the same and of other observers.[38] Again, the requirement of a complete report may be so phrased that the observer understands it, simply, as the demand for a non-selective report; he is to give an account of everything that he can remember, and not to pick out the items that he himself thinks important. The idea that the contents of the after-period are necessarily the same as those of the experimental consciousness may be corrected, as Müller points out, by suitable instruction. On these three points, then, it seems possible to offer a straightforward reply to the critic, and to safeguard the method. The essential thing is, after all, that the method permits and prescribes a truly experimental control of conditions. The advantage is twofold. First, the repeating of the observation, and the varying of its conditions, allow us to test report by report; the method becomes, in a very real way, self-critical:[39] and, secondly, the standardising of conditions fulfills one of the chief requirements of a good method, that the observations may be exactly repeated by other investigators.

But I have said nothing of the perseverative tendencies, the foundation upon which Ach builds. Here we have, in fact, a disputed issue of systematic psychology; and here Ach's method, at any rate

[38] *Cf.* my remarks, *op. cit.*, 505 f., and the references given 503 f. It is noteworthy that E. Westphal, who employed Ach's method (Ueber Haupt- und Nebenaufgaben bei Reaktionsversuchen, *Arch. f. d. ges. Psych.*, xxi., 1911, 432), was careful to phrase his instructions in such a way that "the observers were held down to a report solely of what had, with certainty, been actually experienced" (434).

[39] Westphal's use of the 'synthetic method' (*op. cit.*, 359 ff., 397 f.) is a case in point. After reading certain things *out of* the reports (analytical method), Westphal proceeds to read these same things *into* the instructions of a new experimental series (synthetic method). The reports thus become, in so far, their own test.

in its present form, is plainly inadequate. It may be said, however, that the acceptance of perseveration, as a principle of explanation, is not essential to the use of the method, and that the study of perseveration, as a general term for certain observed phenomena, can be carried on in experiments of less complexity. While, then, it is regrettable that Ach, in the statement of his method, has laid stress upon perseveration,—since by so doing he has exposed himself to criticism which the method does not enable him to meet,—still the method itself does not stand or fall with his personal interpretation, and the rightness or wrongness of this personal view can be determined 'out of court' by other methods. Müller's criticisms do not affect the core of the method, which is, once more, the securing of description under rigorously experimental conditions.

III. I think, then, that the method which is to supplant the method of questions will be of the type of Ach's 'systematic experimental introspection.' And now I return to the method of questions itself.

In his reply to the objections urged by Wundt against the method of examination, Bühler remarks that his first paper was meant to furnish "a preliminary orientation in regard to certain fundamental questions of the psychology of thought."[40] This statement, literally taken, points out the chief use of the method: it enables us to make a first survey, to get our general bearings, in new fields of work. For many years, *e.g.*, experimental psychology looked a little nervously upon such things as doubt, wish, belief, desire, the formations that we now know as 'conscious attitudes;' and the method of examination has brought us face to face with these experiences, and has shown us where the problems lie, even if it has failed to furnish us with a satisfactory analysis. Ach is therefore right in ascribing an heuristic value to the method.[41] It will always be of service where new ground has to be broken, and where the formations are so complex that an immediate recourse to experiment, in the strict sense, is forbidden.

That is, undoubtedly, the chief value of the method; there are, however, other ways in which it may be useful. Thus, we have seen that it is eminently provocative; it is potent to call forth criticism and counter-criticism; if it does not settle questions, at least it cannot avoid raising them. Hence, as the method itself shows where problems lie, so does the criticism which it evokes help toward the formulation of the problems, and show what pitfalls are to be avoided, what precautions taken, when the problems are approached in a properly experimental way. Criticism, I said above, has in general been sharpened to a point too fine for the method and results against which it was directed; within the boundaries of the method, therefore, it has been ineffective and unconvincing; but this same keenness and delicacy will prove of value when the investigator is passing from the method of examination to the other and more refined method of systematic experimental introspection. It is true,

[40] K. Bühler, Antwort auf die von W. Wundt erhobenen Einwände gegen die Methode der Selbstbeobachtung an experimentell erzeugten Erlebnissen, *Arch. f. d. ges. Psych.*, xii., 1908, 103.

[41] N. Ach, *Willensakt und Temperament*, 1910, 16 f. It should be noted that Wundt admits the usefulness of the 'experiment without instruments' in cases where the problems are very simple or where but little work has already been done upon them: *Kleine Schriften*, ii., 1911, 274.

no doubt, that as the coarser method is reduced more and more to a means of preliminary exploration, its provocativeness will steadily decrease. But even if it produce less controversial writing in the magazines, it will continue to work as ferment in the group of co-workers in the laboratory.

I am thus led to a final point. All those who have worked with me by the method of questions have assured me that they have learned from it a great deal of psychology; some have even proposed that it be introduced into the undergraduate laboratory courses. To be set over against a complex experience, and to be asked for a complete description, is—these observers say—variously informing: now, perhaps, one realises the inadequacy of current terms and formulae; now, one learns how much of one's supposed psychology is a matter of borrowed categories or of logical reflection; now, again, one makes novel discoveries as to one's mental constitution; always one is directly impressed by the complexity and elusiveness of consciousness. This advantage must not, I think, be exaggerated. A stricter method might yield the same results; it is not necessary that, because conditions are more rigorously controlled, the experimenter do all the psychologising. And there is, besides, the danger, in this method of questions, that the lines of system harden, and that interpretation be too positive. So far as it goes, however, the judgment of my co-workers must be recorded in favor of the method.

A method which possesses an exploratory value, a critical value, and an educational value,—even if the two latter values are of a lower order than the first,—is not likely to disappear; the prospect, as I remarked at the beginning of this discussion, is that it will extend its range and multiply its forms. But a just appraisal will hardly give it rank with the approved methods of the science. The psychology of the 'higher' processes lies in the hands of *Völkerpsychologie* and of the method of 'systematic experimental introspection.'

PROFESSOR YUZERO MOTORA

Through the kindness of Dr. Hikozo Kakise, a former Fellow and a Ph. D. of Clark University, now in Japan, the AMERICAN JOURNAL OF PSYCHOLOGY has received an account of the funeral ceremonies of the late Professor Yuzero Motora who died December 13, 1912, besides four pictures of Professor Motora at different stages of his life from early childhood up to a period just preceding his death, and a photograph of the funeral ceremonies.

Dr. Kakise has translated and epitomized an address by Professor Sho Watase on Professor Motora's life at Johns Hopkins University, which was one of twenty memorial addresses that are to be published eventually in connection with a number of comments upon his life and character by those who knew him best.

Professor Watase said: "It was in the autumn of 1886 that I became acquainted with Dr. Motora, in Baltimore at Johns Hopkins University, where I was with him for two years, so I will tell you of my personal experiences with him in the university at that time.

"When I first arrived in the city of Baltimore I found Dr. Motora, Dr. Nitobe and Dr. Nagase already studying there. I stayed with them for the first few nights, during which Dr. Motora performed a strange experiment upon me. You may imagine my surprise when, before going to bed, he put on each of us a kind of wire helmet. When I, full of curiosity, asked what it meant, he said: 'During our dreams there occur certain electrical changes in our brains, so if we transmit the electric current from one brain to another by means of the helmet, all of us may have the same dream at the same time.' Though the experiment was repeated several nights we got no noticeable result. It was our opinion that the fault might not be with the doctor's theory, but rather with ourselves, for the helmets often came off while we were asleep as we were restless. His experiment thus ended in laughter, and I have never heard of it since.

"The Psychological Laboratory, where Dr. Motora worked, was then attached to the Biological Laboratory, where Physiology was taught by Professor Martin, Zoölogy by Professor Brooks, and Psychology by Professor Hall, who was Dr. Motora's teacher and who later became President of Clark University. Many young men who were then studying under him have now become famous scholars. Among them are Donaldson, then making a study of the brain; Hodge, of the nerve-cell; Sanford, of experimental psychology; Burnham, of education; Hyslop, of Greek philosophy; and Jastrow, who had finished the university course but was still working in the Psychological Laboratory.

"Dr. Motora had also been appointed Fellow of the University, which was the first in America to adopt this system and which gave annually the sum of ten thousand dollars to twenty fellows, or five hundred dollars to each. The Japanese who had received this honor since the foundation of the university numbered four,—Dr. Kuhara, Dr. Mitsukuri, Dr. Sato, and Dr. Motora. President Gilman was of the opinion that the fellowships should be given only to promising students, and there should be no discrimination of nationalities in the

domains of science. It was chiefly due to this enlightened policy of his that these Japanese were able to share in this benefit.

"Dr. Motora studied Sociology and Economics besides Psychology. He took the degree of Doctor of Philosophy in 1888. By this time he seemed to have gone pretty deeply into the study of Philosophy, and I believe the topic of his dissertation was philosophical rather than psychological, for I remember once hearing a friend advise him against choosing Philosophy as his major and Psychology as his minor subject. The philosophical turn which so strongly marked his subsequent career as a psychologist and teacher undoubtedly took its origin from this period. Dr. Motora's life at Johns Hopkins was doubtless the most successful and the most important of his school-days in laying the foundation for his future studies and career."

Dr. Hall remembers Dr. Motora chiefly as a laboratory student with whom he experimented for a year on the study of "Dermal Sensitiveness to Gradual Pressure Changes," a paper on which was printed in the first number of the AMERICAN JOURNAL OF PSYCHOLOGY, 1888. It was a study that has always been referred to with respect by those who had occasion to treat the subject, and showed remarkable deftness of manipulation as well as ingenuity of method on the part of Dr. Motora. He also participated with Dr. Hall in the study of sleight of hand performances and was able to do a number of very interesting things, and all in a way which showed that he had a very deep interest even then in mystic phenomena. He was, as Dr. Hall remembers him, a man of the most serious and earnest character, quiet and modest, apparently with no other interest whatever than his psychological and philosophical studies, his zest for the latter probably being more pronounced than for the former. On a recent visit to this country, when he was a guest at Dr. Hall's house for a few days, he showed that his interest had gone over very largely into the field of a philosophy that seemed to focus upon religious subjects; and nearly all his conversations were upon a type of religion which should embody and unite the chief truths in the faiths of the Eastern and Western worlds.

Dr. Burnham has especially pleasant memories of Professor Motora as a fellow-student at Johns Hopkins University. He recalls him as a student of the best type, at once sane, temperate, industrious, enthusiastic; as a thinker, distinctly philosophical, bold, original, independent, and vigorous; as a friend, pleasing, reliable, satisfying, and with a large fund of good-fellowship; in character possessing all the sturdy virtues, dependable, trustworthy, dignified, and helpful. He was thoroughgoing and original as a thinker and scholar; and even at that time he possessed apparently distinctly those virtues which make a teacher in the older and larger significance of the word, one whose influence indirectly by character and scholarship may be even greater than his direct influence as an instructor. There is something distinctly wholesome, satisfying, and supporting, even in the memory of Motora's character and good-fellowship.

President Edmund C. Sanford writes: "It is now nearly thirty years since I first met Professor Motora as a fellow-student of psychology and philosophy at Johns Hopkins University. He was a year ahead of me in his university rating and already engaged in research while I was going through the preliminary preparation, and therefore looked up to and envied. Later I served as subject in some of the experiments which he made on sensations of gradual changes in pressure, and still later he kindly assisted me in a similar way in a study of the legibility of letters.

"In the laboratory, in the lecture-room and in the spirited discussions about Dr. Hall's seminary table I came to know Dr. Motora better than I had then known any of his countrymen, and to know him was at once to respect and to feel drawn toward him. Perhaps it was his quiet, reserved and yet friendly manner, admirably fitting the philosopher that he was, perhaps it was his points of view, not always fully grasped by me and having the possibilities of the orient about them, perhaps it was comradeship in the enthusiasm of the place. Whatever it was, it attracted me to him, and fostered a friendship for him which was gladly renewed in 1903 when he contributed a characteristic article to the *Festschrift* for Dr. Hall, and gave me keen pleasure at the sight of him on his recent visit to this country and Europe.

"The individual incidents of our comradeship at the University, so far as I recall them, were for the most part trivial, but the core of one conversation I have never forgotten. It must have been soon after a seminary discussion on the psychology of meditation, mystic contemplation and similar matters—all, to my occidental mind, wholly antithetical to active efficiency. In continuing the discussion afterward, Dr. Motora remarked that many of the most distinguished men of action in his own country were accustomed to practice just such withdrawals from the world of their daily business. His remark forced me to realize, as I never had done before, that there is no necessary antagonism between such states of deep reflection—akin to the essence of prayer—and vigorous activity; indeed that deep contemplation may itself furnish at once the inspiration and the supreme guide of executive efficiency.

"I suspect that Professor Motora himself, though a philosopher and scientist rather than a man of affairs, may have been speaking in part from his own experience, and that the practical regimen of this combination of contemplation and action may be one of the lessons which the West may learn with profit from the East."

A CORRECTION

I wish to take the blame for the mistake which Ferree (on pp. 379-380 of this number) attributes to Geissler. So far as I recall, the facts are as follows. When Ferree consulted me with regard to a method of stimulation of the tongue, I suggested Christmas-tree foil. As I heard nothing more of the matter, I supposed that this material had been used. Ferree was ill at the time that his article was published, and I had the responsibility of proof-reading. The sentence "Strips of very light tin foil were used as electrodes" (this *Journal*, xvii, 1906, 119) was changed by me by the insertion of "(Christmas-tree foil)" after "foil;" I thought that the naming of the material might be useful to others. The mistake caused Geissler a good deal of trouble,—for which, as well as for the misrepresentation of Ferree, I am very sorry.

E. B. TITCHENER.

FIFTH REPORT OF THE POLISH PSYCHOLOGICAL SOCIETY

Mr. E. Abramowski was appointed as director of the Psychological, Dr. E. Flatau as director of the Neurological Laboratory.

The Society arranged 15 meetings, at which the following papers were read:

Dr. Joteyko-Rudnicka, Report of the Work of the Psychological Laboratory at the University of Brussels; Dr. Ada Silberstein, Ugliness and Esthetics; Dr. Fl. Znaniecki, Efficiency in the Light of Pragmatism; St. Lesniewski, The Problem of Existence in the Light of Grammatical Norms; Dr. Josephine Kodis, Metaphysics in Science; Dr. Vincent Lutoslawski, Integral Psychology; Dr. W. Radecki, Psychoelectrical Phenomena; Dr. W. Tataskievicz, Apprehension and Judgment; Dr. Cygielstreich, The Influence of Emotions on the Development of Brain Diseases; Dr. Tl. Znaniecki, Practical Reality; T. Kievski, Philosophy in the Normal School (Gymnasium or Polish School); Dr. T. Halpern, How to Understand "The View of the Universe"; St. Lesniewski, Contradictory Views on the Theory of Language (Speech); Dr. B. Bornstein, The Euclidic Character of Space; Dr. J. Ochorowicz, Experimental Tautology.

Researches of the Psychological Laboratory:

E. Abramowski, Analytical Telepathy as a Phenomenon of Cryptomnesia; The Influence of Fixed Attention on the Breathing and Pulse; Influence of Will in Emotional States, Measured by the Galvanometer; The Connection of Emotion in Reference to Memory; A. Cygielstreich, Subconscious Creativeness in Children; Subconscious Creativeness in Adults; Mary Lipska, Connection between Discriminative Perception and Vocational Fitness; W. Makowski, Emotional Coördinations Measured by Galvanometrical Reactions; T. Lapinski, Subconscious Creativeness in Mental Diseases; J. Warszavska, Subconscious Creativeness in Children.

Researches of the Neurological Laboratory during the year 1911: Dr. M. Bornstein, Changes in the Spinal Cord Induced by Depression; Studies on Landrié's Disease; Dr. Flatau, The Parallel Between Functions and Real Changes in the Nervous Fibres; Dr. Flatau and Dr. Handelsmann, Analytical Experiments as to the Treatment of the Inflammation of Nerve Centres; Dr. Gepner, The Influence of Natri Nucleinici and Salversan on the Nervous System of Animals; Dr. Handelsmann, The Treatment of Neuralgia; Experimental Works on the Brain, etc.; Dr. Koelichen, Chromatophorma Medullae Spinalis; Studies on the Anatomy of Lymphatic Lines, etc.; Dr. Rotstadt, Studies on Myasthenia; Dr. Synechowicz, Studies on the Pathological Anatomy of Alzheimer's Disease; Studies on Changes in Parkinson's Disease; Studies on Meningitis, Cerebro-spinalis epidemia, etc. Women students were working in this Laboratory chiefly as *laborants*.

CONVENTION OF EXPERIMENTAL PSYCHOLOGISTS

The tenth annual conference of Experimental Psychologists was held April 10th to 12th, at Wesleyan University, as the guests of Professor Raymond Dodge. The first two sessions were occupied by the reports of the investigations in progress at the various laboratories represented, about twelve in number. At another session, the directors of several of the larger laboratories gave a résumé of their own work during the last ten years, since the starting of these conferences, and discussed the advances made in the general field of psychology during that time. Some mention was also made of the problems which it seems desirable to attack in the near future.

At one of the sessions there was a lively discussion as to the "Behaviorist" viewpoint recently advanced by Professor J. B. Watson. Those present were surprisingly in accord against such an envisagement of the problems of psychology. Unfortunately Professor Watson was not present to defend his position.

One session was devoted to tests—their theory, value and limitations—in which papers were presented by Dr. F. L. Wells and Dr. H. H. Goddard, although the latter was unable to be present in person. In the discussion, the general opinion seemed to be that the word "test" was a misnomer; as those now applied do not give indications of the degree of mentality of the subjects with any amount of finality. The question of the attitude of the experimentalist towards those who may attempt to apply psychology to business, industry, vocational guidance and similar practical problems, was brought up and discussed in some detail. The general opinion seemed to be that such applications had better come directly from the universities rather than from independent workers on the outside.

Professor Hugo Münsterberg delivered a semi-public lecture on his recent investigation of the mind reading of Beulah Miller, which aroused a great deal of interest.

The social features of the meeting included a luncheon, tendered by the University, and a dinner at the home of Professor Dodge; as well as a smoker at the University. On this latter occasion Professor Dodge demonstrated some of his apparatus, which was most ingenious indeed.

It was decided that the next meeting be held at Columbia University in the spring of 1914.

Clark University. S. W. Fernberger.

BOOK REVIEWS

The Place of Illusory Experience in a Realistic World. By EDWIN B. HOLT. In *The New Realism.* New York, The Macmillan Co., 1912. pp. 491. Price $2.50 net.

The main contention of the author of this essay is that all mental phenomena are the same in kind as the phenomena of the physical world. He argues (1) that this thesis holds for sensation, perception and image; and (2) that contradictions, which constitute the content of an illusory experience, are not subjective, but objective, in nature. He devotes the greater amount of his space to the development of the first of these two arguments, which is also the more interesting to the psychologist.

Errors of space and of time receive scant attention. It is enough to show that for each of them an analogous phenomenon may be found in the physical world. If one presses an eyeball out of place, and keeps both eyes open, one will see double; so does a stereoscopic camera. "Or, again, an astigmatic eye distorts its object; so does a roughly cut glass lens." We see some heavenly body many years behind time: what advantage over us has the photographic plate? Many of the errors in secondary qualities are parallel to those of space and time; but the complementary after-image and the pure hallucination offer greater difficulty. The former is met by a new theory of vision based on Meisling's view that the cones resonate to waves of light. It has been found that, when the capacity of the receiving mast of a wireless telegraph system is tuned to a given length of Hertzian wave, it is *ipso facto* tuned to a second wave-length as well, which may be regarded as its complementary wave. Light waves and Hertzian waves are closely related physically, so that, if the view of Meisling is correct, the complementary colors of vision are nothing but a true presentation in consciousness of the fact of complementary attunement of light-resonators. The case of pure hallucinations raises two questions: (1) How can these purely hallucinatory secondary qualities have any sort of being other than a subjective and mental being? and (2) How can they assert *themselves to be* or how can the *realist* pretend to *assert them to be* the real object? The answer to the first question is long and involved. The doctrine of specific nerve-energies is first attacked, on the ground that physiologists have been unable to discover any trace of specific nerve-energies in the differences between nerve-impulses, nerve-fibrils, cortical cells and synapses. Furthermore, a satisfactory theory of specific nerve-energies must furnish an explanation, not only of qualitative differences between sense modes, but also of qualitative differences within sense modes. No theory, not even Helmholtz' theory of audition, meets this demand with success. Specific nerve-energies must, therefore, be given up. The author admits that there are qualitative differences; but he thinks that they can be analyzed into quantitative differences, and for this reduction he carefully prepares the way. Physiologists have found that the nervous impulse presents periodic fluctuations of a frequency much higher than had previously been suspected. The nervous response follows the rate of stimu-

lation, and the periodicity of the impulse may rise as high as one thousand in the second. This discovery is accepted as the physiological basis of the author's explanatory psychology. Recent experiments in audition, particularly those of Lord Rayleigh, point to a vibratory theory of audition; and a combination of the Rutherford and Meyer theories proves adequate to the facts. In the field of vision, the resonance theory falls readily under the new point of view. The light waves may act photo-chemically on the visual purple of the rods, and physically on the cones. Still, of course, the question remains: How do the periodic vibrations become qualities? And the answer is that the qualities are a kind of form-qualities, "in which the temporal subdivisions are so small that the time-sense cannot discriminate them, whereas the frequency-magnitude, or the *density*, still remains perceivable." But it is not the factor of density of the nervous impulse that is the secondary quality; it is rather the density of the series of some relatively primitive sensation which is the secondary quality. As form-qualities these densities have two characteristics which differentiate them from other form-qualities: (1) their principle of organization is time, and (2) they are of a lower order than the *Gestaltqualitäten* with which we are familiar. The task of psychology, now, is to analyse all form-qualities, as far as possible, by introspection. Of the original five senses, taste and feeling have already proved to be fusions; the clang can be analysed into partials, orange into red and yellow, and even green is phenomenally yellow and blue. When, however, introspection can go no further by its usual method, it may try a new method. A quality that lies in a series between two others may be eliminated, on the ground that it possesses the same ingredients as these two. Eventually, then, we shall get down to a single element, such as one kind of atom, variously organized, such as in three sizes of molecule. This is the primitive entity whose density constitutes a secondary quality. It is not mental or subjective in substance; on the contrary, it is the same in substance as the physical element.

This reduction completes the author's first argument. Realism is able to assert the reality of an hallucination, because the nervous system is able to generate within itself nerve-currents of frequencies whose density-factor is the same as that of ordinary peripheral stimulation. It remains to say a word concerning his theory of consciousness. "Consciousness is the group of (neutral) entities to which a nervous system, both at one moment and in the course of its life history, responds with a specific response." Either the object, or the color (secondary quality) on the object, is *specifically responded to* if the nervous system can pick up and transmit the vibrations which are sent out by it. Consciousness, moreover, is not in the skull; it is out there in space, precisely where it appears to be.

The author next takes up the problem of error. No thing, be it brickbat or image, can of itself assert anything about reality or unreality. Such a bare content is in logic known as a 'term.' A term, or system of terms, simply is, and is neither true nor false. Contradictions, however, may exist among sets of assertions or propositions about a term. A proposition is supplied by experience, and, if it be about a mental term, may coexist with this in the mind. Terms in relation which are physically impossible, as for example the round square, are also mentally impossible, *i. e.*, unthinkable. But the mind may entertain contradictory propositions about the round square; and physical laws, which are propositions, are habitually in a state of contradiction, as for example when two laws of motion oppose each other.

"A thought, then, which negates another thought is neither more nor less significant than a physical law which negates another physical law."

We have tried to give an abstract of the author's views as we understand them. We have not indicated in any degree the ingenious way in which the argument is presented, nor have we hinted at the apt illustration, the pleasing metaphor, the friendly sarcasm which abounds throughout. Our principal objection is that he writes as an advocate, and not in the dispassionate way in which scientific theories should be discussed. The acceptance of the argument as it stands means the acceptance of new theories of vision and audition, of a new kind of form-quality—to say nothing of the old, of a thorough-going atomism, and of a strange theory of consciousness. The author would agree that the only test of a theory is an appeal to the facts; we wish that he had at least indicated how his theory of vision would explain simultaneous contrast. Again, it is not quite fair, where there is some disagreement as to what are the facts, to choose one set of observations and entirely to ignore the others. For example, there are psychologists, who are aware of the stimulus-error and who work with spectral colors, and not with pigments, who still insist that orange is a simple quality. There is also good reason for believing that the rods are organs of night vision only. In which case the cones, according to the novel theory, must reasonate to white light. What would a resonator, tuned to a pair of wave-lengths, do with so complex a wave as that of white light? The author cannot say that each resonator picks out its own wave-length, for the retina would then be an analyser, and that would imply specific nerve-energies. Moreover, we should, in such case, be able to analyse mentally white light as we do a clang. Similar objections might be raised against the theory of hearing: we will only remark that Wundt, twenty years ago, considered such a theory, but declined to accept it because it cannot satisfactorily explain clang-analysis. As regards the view of secondary qualities as densities, one can only speculate. The sole evidence which the author brings to the support of his theory, the roughness and smoothness of the tapping experiment or of the flicker experiment, is hardly germane; for roughness and smoothness are not simple qualities, as is the quality of a color or a tonal sensation. If, however, we were to hazard a guess, we should agree with Montague that the author has given us an interesting explanation of intensity, but not an explanation of quality at all. As to form-qualities there is still the possibility, as Titchener has pointed out, that the problem of meaning is involved; so that, just as no content can of itself assert anything, so none can mean anything,—not even triangularity.

Cornell University. H. P. WELD.

Das Problem der Willensfreiheit. Von G. F. LIPPS. Leipzig, B. G. Teubner, 1912. pp. iv, 104.

This booklet, published in the series "*Aus Natur und Geisteswelt*," is based on *Volkschulvorträge* delivered by the author at the University of Leipzig. Lipps accepts Kant's statement of the problem of freedom: that man is at once free and determined; but he seeks a solution more adequate than that of Kant. Instead of splitting hairs over the meaning of Freedom and Determinism, the author proposes to examine the essence of our own being and conduct, in an endeavor to solve thereby the puzzle of the free-determined character of our willing and acting. To this end, he presents in brief outline the part

played by reason in various Greek theories; Augustine's doctrine of the good and the bad will, and the modern rationalistic accounts of the relations of mind and body. Kant, he thinks, is enabled to hold causality and freedom side by side, only by lifting morality out of the world of phenomena, and thus making man a member of two universes. The relation between these two human natures remains a mystery for German idealism; that mystery Lipps seeks to grasp in terms of actuality. As thinking beings, he holds, we are convinced of the thoroughly necessary character of human conduct. Man must act in the way in which he does act. The preconditions of each act, however, can never be completely indicated; and this inevitable residuum of uncertainty leads us naïvely to postulate a will essentially free. Thus the problem of freedom and determinism, Lipps concludes, involves the discrimination between the naïve and the critical attitude towards human conduct.

Clark University. RADOSLAV A. TSANOFF.

Ethics. By G. E. MOORE. New York, Henry Holt and Company; London, Williams and Norgate, 1912, pp. 256.

The aim of the author is "to state and distinguish clearly from one another . . . the most important of the different views which may be held upon a few of the most fundamental ethical questions" (p. 11). This he attempts to do by devoting a third of his book to an analytic statement of utilitarianism, and the remaining two-thirds to an examination of possible criticisms of that theory. Moore defends utilitarianism against the line of attack which consists in saying that right and wrong are merely subjective predicates. To say that an action is right or wrong, intrinsically good or bad, is not to say that one has towards it any mental attitude whatever. Nor is there sufficient reason for accepting as the test of right and wrong the intrinsic nature of the action, the motive prompting it, or its probable consequences. The discussion of free will "concludes with a doubt" (p. 222). The egoistic objection to utilitarianism is likewise rejected; but after disposing of all other criticisms of that theory, Moore advances his own, which he considers fatal; utilitarianism claims that rightness and wrongness depend on the intrinsic value of the consequences of our actions, and yet it does not rightly decide what constitutes intrinsic value.

The book is intended for the lay reader; the continual iteration of apparently simple ideas seems to indicate that the author was perhaps too well aware of his task as popularizer. The central place which is given to utilitarianism doubtless provides a convenient opportunity for presenting the author's own point of view in minute and finely spun reflections on hedonism; but it leaves the reader with a wrong historical perspective of ethical theory. The method of exposition, also, is unfortunate: the popular reader is offered hairsplitting distinctions and abstract explanations in terms of the conventional *A* and *B*. Unduly replete with technicalities which are out of place in an elementary treatise, and lacking almost any concrete illustrations from daily life, this book is at once too subtle and too dry for its purpose.

Clark University. RADOSLAV A. TSANOFF.

Elemente der Völkerpsychologie. Grundlinien einer psychologischen Entwicklungsgeschichte der Menschheit. Von WILHELM WUNDT. Zweite unveränderte Auflage. Leipzig, A. Kröner, 1913. pp. xii, 523. M. 14.

The monumental volumes of Wundt's *Völkerpsychologie* find not only a summary but also a crowning supplement in the *Elemente der Völker-*

psychologie. Here the entire mental history of man is outlined in a continuous narrative; the various activities, such as custom, myth and religion, and art, are not separated but dealt with in conjunction. We pass from 'Primitive Man' to 'the Age of Totemism,' then to 'the Age of Heroes and Gods,' and finally into 'the Development toward Humanity.' It is safe to say that no other man could have told the story as Wundt has; his vast learning, powerful psychologic insight, vivid sense of history, and, not least, his stylistic ability to present states of flow and change have produced a work of tremendous and awing effect.

It is not necessary to recall here the justification of a social psychology as an inevitable consequence of the rejection, by empirical science, of the metaphysical postulate of an individual soul as the substratum or receiver of experience.[1] Familiar, also, are the general results of the social psychological method: the exclusion of reflective rationalizing explanations, in which logical processes are falsely projected by the explainer into the communal developments. In the *Elemente* one can see the larger results of this methodic precaution. Nowhere is a reflection about the consequences of a development assumed as the cause of that development. Hence the frequent reversal of the naïve view of things: the demon is not a causal explanation of natural happenings, but a creation of emotions, especially fear (p. 355), for the primitive knows only magic causality (pp. 90 ff.). Again, the god grows out of the demon, religion out of the beliefs in demons and spirits, and law out of custom. All this is too familiar to require comment.

In one respect, however, the *Elemente* differs, even externally, from the *Völkerpsychologie*. In the latter the subject of language receives two volumes, placed at the beginning of the work,—naturally enough, for language is 'the universal substratum of mental culture' ('die allgemeine Trägerin der geistigen Kultur,' *Elemente*, p. 487). And it appears, at first, when one studies these volumes, that Wundt's social psychology has done for our knowledge of linguistic development exactly what it has done for the other spheres of social activity. Especially the processes of linguistic change had been interpreted only too much as if they were acts of logical reflection; by putting an end to such interpretation and showing the concrete psychological character of changes in language, Wundt has done an inestimable service to the science of linguistics. In the *Elemente*, however, we find but a few pages in the division on 'Primitive Man' (I, §§ 5 and 6) devoted to language. A sketch of gesture-language and one of a supposedly rather 'primitive' language, the Ewe of Togoland, is all we receive. Of the development from lower to higher forms, or even of any criteria of distinction between these, we learn nothing. This is due to the fact that minute analysis of the processes of change is excluded from the *Elemente*. Descriptions—and Wundt is a master of what may be called kinetic description—of typical stages of the social institutions suffice for the purposes of this book. Could they not have been given for language also? In the *Völkerpsychologie* Wundt has contributed much toward the detailed psychological interpretation of the processes of linguistic change, but toward a history of the development of language ('die generelle Entwicklung') he has given little. The origin of language is splendidly treated and there are valuable ideas and discussions which have bearing on the general further evolution of language; but an outline of this evolution or even a sufficient indication of the direction of

[1] *Völkerpsychologie*[3], I, 1, p. 9; *Grundriss der Psychologie*[11], § 2, 1.

development there is not. This, I believe, accounts for the scant treatment of language in the *Elemente*, where only such an outline, with portrayal of typical stages would have been in harmony. Toward this we find in the *Elemente* only a sketch of the origin of vocal language in the light of gesture (Wundt's greatest single linguistic contribution lies here), and then the description of a 'primitive' language.

To regard Ewe as such is, however, a mistake. Language, like the other communal activities, changes most rapidly where there is most contact of communities,—where there are wars, migrations, and, above all, transferences of language to new peoples; and Africa, everything indicates, has long been the scene of all these happenings (*Elemente*, pp. 136 ff.). Further, Ewe is spoken by several millions of people and even serves as a literary language.[2] It is a member, moreover, of the widespread West-Sudanese family of languages, which possibly may be related to the other great family of the Bantu languages. Plainly, then, this language has behind it a long history of spread, migration, and change. This becomes a certainty when we learn more of its forms. It is a nearly monosyllabic language: the languages of whose history we know anything show a constant shortening of the word toward monosyllabism. This is the direction in which the languages of Europe have developed, especially, of course, English. When, further, we learn that in Ewe the word-order is fixed, we must entirely refuse it the title of a 'primitive' language, for linguistic history everywhere shows us that the syntactic utilization of word-order is a gradual accomplishment. Wundt thinks it a primitive characteristic that in Ewe the modifying word follows its subject ('man big,' not 'big man'), as in gesture-language. It is obvious that fixed word-order allows of only two possibilities, the one realized in Ewe, as in modern French (where we can see it growing out of the free word-order of Latin), the other, for instance, in modern English and in Chinese. Of the Indo-Chinese family of languages,—divergent modern forms of a single older speech,—Chinese and Burmese let the modifier precede, Tibetan and Siamese let it follow. In other words, the correspondence of the Ewe word-order with the order of gesture is, for the question in hand, accidental. When Wundt further cites lack of inflection as a primitive characteristic, he runs directly counter to the evidence of all known linguistic history. Wundt further cites the prevalence of what is called sound-symbolism, but this, again, is a feature which we see growing in some highly developed languages, notably German and English,—as Wundt admits (*Elemente*, p. 67). The symbolic words,—such as, for instance, in English, *clash, crack, crunch, sputter, splutter*,—originate by the same processes as other words and can by no means be cited as traces of the birth-hour of language. No objection can be made to the statement that the method of expression in Ewe is highly concrete. It is an accepted doctrine, however,—and one supported by Wundt's own chapter on Semantic Change in the *Völkerpsychologie*,—that abstract expressions develop at need out of concrete, provided the individualization of concepts (cf. below) has once taken place. A language like Ewe has no highly abstract expressions because the people who speak it have no occasion to speak of abstract matters. Should the occasion arise, the words would soon find themselves.[3]

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[2] Finck, *Die Sprachstämme des Erdkreises*, p. 119; cf. also Cust, *The Modern Languages of Africa*, pp. 203 ff.

[3] Cf. especially F. Boas, *Handbook of American Indian Languages*, I, 64 ff.

The source of Wundt's error lies in the fact that, to repeat, his social psychology does not contain in regard to language a view of the general development comparable to that of the other fields of social activity. It would be needless here to give a detailed statement of Wundt's views on points related to this question. Suffice it to say that the rationalizing interpretation, which here also reverses the true course of development, is not entirely overcome in the *Völkerpsychologie*. In its extreme form this interpretation sees 'primitive' language as a system of monosyllabic words, each with a separate conceptual content; in the course of development these gradually merge into an 'agglutinative' state, in which a number of them lose their full conceptual value and become modifying affixes; finally the syllables of such groups lose their formal and semantic identity still more, until polysyllabic inflected words, like those of Latin and Greek, result. As the historically observable course of events is always diametrically opposed to this, the auxiliary supposition becomes necessary that development continues only until a language 'enters into history' (by being recorded in writing), at which point there begins a period of 'decay.' This theory, developed chiefly by August Schleicher, may be seen in its application to the various languages of the world in A. Hovelacque's book, *La linguistique* (fourth edition, Paris, 1888). The selection of Ewe as a typical 'primitive' language,—even though Wundt is far beyond the grossness of such theorizing as I have described,— is nevertheless a reflex of such views. Actually,—that is, wherever the facts are accessible,—language is always seen to develop from longer words to shorter, from words involving more experience-content to words of simpler conceptual value. Or, more correctly, the sentence of imperfectly analyzable associative structure, whose parts merely resemble parts of other sentences, gives place to the sentence fully analyzable into separately apperceptible units (words) which are felt to occur with unchanged identity in other sentences. It is this contrast which really embodies the linguistic phase of Wundt's statement (*Elemente*, p. 73): 'So ist das Denken des Primitiven fast rein assoziativ. Noch ist die vollkommenere Form der Verknüpfung der Begriffe, die apperzeptive, die den Gedanken in ein Ganzes zusammenfasst, nur spurweise in der Verbindung der einzelnen Erinnerungsbilder vorhanden.' We must, accordingly, mark as most primitive those languages in which the sentence scarcely or not at all breaks up into words, but is analyzable only as an associative complex, in the sense that parts of it resemble parts of other sentences. Thus a language in which 'I-cut-bear's-leg-at-the-joint-with-a-flint-now' is a single highly inflected word[4] is a relatively primitive language. By the same token Latin *ambulo, ambulas, ambulat, ambulabam* are as sentences more primitive than, say, the English equivalents *I am walking, you are walking, he (she) is walking, I was walking*, because the English sentences consist of several independent symbols each with conceptual value (words), while the Latin expression views each occurrence as a whole, with only associative indication of resemblance to other occurrences.

The importance of a proper understanding of these things for the mental history of man is not only guaranteed by the function of speech as the substratum of communal mental life, but follows immediately from the nature of our concepts of quality, action, and relation. These,—as no one has, to my knowledge, better described than Wundt (*Völkerpsychologie*[2], I, 2, p. 513 ff.),—depend for their existence upon

[4] R. A. Marrett, *Antropology*, p. 139.

a separately apperceived object of symbolic value which serves as dominant element in the complex forming the concept. This symbol-object is, of course, the word: without it no concept of action, quality, or relation can exist. Hence, without independent words for such ideas, no scientific thought is possible. The central thread of the mental history of man is a development whose most immediate external manifestation is the attainment of linguistic symbols for concepts other than those of objects. L. Lévy-Bruhl, in his *Fonctions mentales dans les sociétés inférieures* (Paris, 1910), has observed the connection between primitive habits of thought and non-isolating habits of speech. So much is certain: no people, so far as is known, has arrived at what may properly be called logical or scientific thought without speaking a language at least as far along toward conceptual expression as Sanskrit, Ancient Greek, or Latin. The 'magic causality' of the savage becomes fully intelligible only when we learn that his thought lacks the linguistic forms which make possible our logic. I shall quote a few passages from the *Elemente* which, now more or less parenthetic, could, by a juster and fuller treatment of the evolution of language, have become integral, and, I venture to think, central motifs of the discussion. Pp. 91 ff., especially p. 93: 'Kausalität in unserem Sinne existiert für den primitiven Menschen nicht. Will man auf seiner Bewusstseinsstufe überhaupt von dieser reden, so kann man nur sagen: ihn beherrscht die Zauberkausalität. Diese aber bindet sich nicht an Regeln der Verknüpfung der Vorstellungen, sondern an Motive des Affekts.' P. 463 f.: 'das Heldenzeitalter . . ., dessen Grundstimmung die Gebundenheit an die objektive Welt ist, in die zwar das Subjekt seine eigenen Gemütsbewegungen hinüberströmen lässt, die es aber niemals von den Objekten zu lösen vermag . . .'

University of Illinois. LEONARD BLOOMFIELD.

The Measurement of Induction Shocks. A Manual for the Quantitative Use of Faradic Stimuli. ERNEST G. MARTIN, Ph. D. New York, John Wiley and Sons, 1912, vii. pp. 117.

This is a collection and systemization of a series of papers published during the last five years. In physiology to-day there is a great deal of work with the artificial stimulation of tissue, and induction shocks are usually used for this purpose. For quantitative work it is necessary to have an exact measurement of the intensity of the shock in order to control one's own experiment or to repeat those of some other investigator. This book is an exposition for the calibrating of induction apparatus so that the value of the shocks may be expressed in stimulation units and so that the calibration can be determined in any ordinarily equipped physiological laboratory. Martin does not present a new method but rather an extension and systemization of other methods of recognized worth.

The factors which may affect the strength of the faradic current are: I. Variations in the primary coil, due to (1) the amount of current yielded by the source; (2) the key whereby the current is made or broken. II. Variations in the secondary coil, due to (1) the position of the secondary with relation to the primary coil; (2) the electrical resistance of the tissue which is being stimulated; (3) the contacts between the stimulating electrodes and the tissue to which they are applied. These factors can all be determined mathematically and a clear and lucid explanation is given of the determinations of these variables. Besides different inductoria present structural differences which may cause variation, due to (1) the dimensions and

the number of turns of wire; (2) the presence or absence of the iron core; (3) the difference in physiological shock between the make and break. These are also determinable mathematically and the methods for obtaining these determinations are given. There is also another variable factor considered which, however, is not capable of mathematical determination: the effect on the stimulus of the manner of making and breaking the primary current. Although the effect of this factor may not be calibrated, still rules are given by means of which it may be kept constant.

The author gives a straightforward description of the procedure for making these various determinations with only enough theoretical material so that these procedures may be clearly understood. A short, concise description of the various apparatus and devices used is also included along with very helpful diagrams. The reader need not fear being plunged into a complicated theoretical mathematical discussion as the book succeeds in what it purports to be,—"a manual rather than an exposition of principles."

Clark University. SAMUEL W. FERNBERGER.

The Belief in Immortality and the Worship of the Dead. Vol. I. The Belief Among the Aborigines of Australia, the Torres Straits Islands, New Guinea, and Melanesia. By J. G. FRAZER. The Gifford Lectures, St. Andrews, 1911-1912. London, Macmillan & Co., Ltd.; New York, The Macmillan Co., 1913. pp. xxi, 495. Price, $3.25, net.

Dr. Frazer, who has always been interested in the attitude of primitive peoples to their dead, has here brought together such information as is available upon the subject of his title. The book does not, perhaps, offer much that is new to the student of social anthropology who has followed the course of the science since 1890, or even since 1900; it is rather surprising—and in view of the rapid disappearance of "aborigines" everywhere, it is reassuring—to note how many of Dr. Frazer's references are of quite recent date. The evidence is marshalled, however, with great literary skill; though the task which the author has set himself is purely descriptive, comparison is not altogether lacking; and once in a while we are treated to an excursus such as readers of The Golden Bough have come to expect. Some of the chapters are a trifle gruesome, since primitive man, like the lower and middle classes of more civilized societies, rejoices to manipulate and decorate a corpse; but we have grown used, of late, to plain speech in matters anthropological; and plainness of speech is necessary if we are ever to understand.

Dr. Frazer believes that "the worship of the human dead has been one of the commonest and most influential forms of natural religion, perhaps, indeed, the commonest and most influential of all" (p. 23). The statement contrasts squarely with Eduard Meyer's dictum: "in Wirklichkeit spielt der Totendienst in der eigentlichen Religion bei den meisten Völkern eine sehr geringe Rolle." It is true that Dr. Frazer takes the word "worship" in a wider sense than Professor Meyer; and it is true that in Australia and the Torres Straits he finds "germs" and "elements" of worship rather than worship itself,—in British New Guinea "a real worship of the dead, *or something approaching to it*" (italics mine), in Dutch New Guinea "something which might almost be called a systematic worship of the dead,"—and that only when he reaches New Caledonia does he allow himself the positive statement: "on the whole we may conclude that among

the natives there exists a real worship of the dead" (p. 338). Yet the contradiction, if lessened, is still not removed; and it is well to remind ourselves that "the worship of the dead" is a slippery and controversial phrase. Controversial, too, is the question of the relation of magic to religion; Dr. Frazer, as is but natural, sees his own view confirmed in cases where another interpretation appears possible. In such matters, difference of opinion, in the present state of our knowledge, is inevitable, and a clear-cut hypothesis has at any rate the value of a fixed point of rally and attack. I could wish, on the other hand, that Dr. Frazer might some day break loose from associationism, which as psychology is outworn and as theory of knowledge is, I imagine, in no better case. We shall not solve the puzzles of the primitive mind until we approach it by way of a sound general psychology.

The impression which the book leaves is that of the tremendous consequences—moral, social, political, economic—which the belief in human immortality has brought in its train: once again, in the history of science, a remote and curious study proves to be of great "practical" importance. In detail, the volume is full of interesting things. Ghosts in Central Melanesia are "naturally in a dazed state at first on quitting their familiar bodies" (p. 358); and ghosts in the civilized world, if we may trust the mediums, suffer a like disability. Among the Melanesians, again, faith (quite logically) kills as well as cures; the natives of the Banks Islands have invented a portable ghost-shooter,—which sometimes hits the wrong man; and the Fijian, learning that he is the object of "malicious animal magnetism," lies down and dies (pp. 387, 414). Interesting from another point of view are the questions to which, as yet, no answer can be returned: "the whole question of the meaning of burnt sacrifice is still to a great extent obscure" (p. 349); and there are many special (p. 462) and more or less general (p. 428) practices of which the same thing must be said.

The printing of the book is excellent. Aside from a few minor slips in the foot-notes, I have marked only one misprint: *consumeri* for *consumere* on p. 346. E. B. T.

The New Philosophy of Henri Bergson. By EDOUARD LE ROY. Authorized translation by Vincent Benson. New York, Henry Holt & Co., 1913. pp. v-235.

In the first division of this book, Le Roy discusses the method and the teaching of Bergson, giving a survey of the problems of immediacy, duration, perception, liberty, evolution, consciousness, life, matter, knowledge; while in the second division, he treats each of these problems in a more detailed and critical manner. Le Roy conceives the critic's task to be one of interpretation of the spirit of the work, rather than to be one of mere enumeration of contents. He believes that misunderstandings to be feared, should be pointed out and anticipated. That Bergson believes Le Roy to have accomplished this we learn from a statement, quoted in the introduction, which Bergson made to Le Roy—"Underneath and beyond the method you have caught the *intention* and the *spirit*. . . . Your study could not be more conscientious and true to the original." That Bergson has not over-praised becomes clear upon a reading of this masterful little presentation of Bergson's philosophy, but what is perhaps not made so clear is the proof of Le Roy's statement that the readers of Bergson "will find the curtain drawn between themselves and

reality suddenly fall and reality stand forth fully revealed." Rather, we should say, that if the curtain is to fall it will more probably do so after a reading of Le Roy than after reading Bergson. The author regards Bergson's philosophy as effecting a revolution equal in importance to that effected by "Kant or even Socrates." For him this philosophy is not a poetical delusion but the result of exhaustive research. Whether we give or refuse complete or partial adherence to it, we have at least all received from it a regenerating shock. He finds it to be in accord with the tendency of the age to question the justification of a deification of science, to distrust the adequacy of intellectualism and to employ, instead, a method of complete experience. To-day everything is regarded from the point of view of life, and there is a tendency more and more to recognize the primacy of spiritual activity.

The following are the main points of Bergson's philosophy, as interpreted by Le Roy. Common-sense (the result of intelligence "living, working, acting, fashioning, and informing itself") before the rational and perceptive function has emerged, is prepossessed in a practical direction, and accordingly has subjected the facts of primal intuition in the direction of utility. Now, even perception, in the usual sense, means the resolution of a problem, the verification of a theory. But we must so mobilize our perspective faculties that we become capable of following all the paths of *virtual* perception, of which the common anxiety for the practical has made us choose one only. Philosophy must free intelligence from these utilitarian habits by endeavoring at the outset to become clearly conscious of them. In order to come into immediate contact with reality, philosophy must renounce the usual forms of analytic and synthetic thought and achieve a direct intuitional effort. "This does not mean to quit experience, quite the contrary, but to extend it and diversify it by science; while, at the same time, by criticism, we correct in it the disturbing effects of action and finally quicken all the results thus obtained by an effort of sympathy, which will make us familiar with the object until we feel its profound throbbing and its inner wealth." This intuition is not a mysterious, mystical thing; it is a method of going from things to concepts,—of incessantly creating new thoughts and incessantly recasting the old. It is thus in opposition to the cinematographic method of analysis by concepts, which, though it presents ever so large an accumulation of conceptual actions, will never reconstruct the movement itself. There is one case in which this "sympathetic revelation" is almost easy to us,—that is in the case of ourselves. This inner world is one of pure quality. In its true nature it is an uninterrupted flow; it is ceaseless change. It is becoming, progress, growth; it is creative process which never ceases to labor incessantly; it is *duration*. Man is free when his acts proceed from his entire personality. In the study of external nature, also, if we do away with the ready-made concepts of mechanism and finality, we find that vital evolution is a dynamic continuity, a continuity of qualitative progress; it is essentially duration, an irreversible rhythm, 'a work of inner maturation.' By the memory inherent in it, the whole of the past is forever present in it . . . that is, it is experience. It perpetually invents, defying alike anticipation and repetition. The vital impulse consists in a "demand for creation." "Consciousness as the original and fundamental reality, always present in a myriad degrees of tension and sleep and under infinitely various rhythms, is present everywhere. Its effort sends out a current of

ascending realization which again determines the counter-current of matter. Thus we should not speak of mind and matter, but rather of spiritualization and materialization, the latter resulting automatically from a simple interruption of the former." Thus, in a word, the philosophy of Bergson is a philosophy of *duration*.

Especially valuable are Le Roy's answers to many of the sweeping criticisms that have been made of Bergson's philosophy. Philosophical intuition is not "aesthetic intuition": it is not a return to Romanticism, but a new logical method. "It does not seek to attain knowledge by renouncing intelligence, placing it under tutelage, or abandoning it to the blind suggestions of feeling and will, but seeks to reinforce intelligence by the initial resources (now represented in instinct), which under the dominance of practical demands it has sacrificed." The philosophy of Bergson does not abolish the problems of morality and of the intellect.

Although a reader can hardly agree with many statements which the author's enthusiasm and complete acceptance of Bergson's philosophy leads him to make, and although he is not bound to accept the statement that this is a new philosophy (for he may rather agree with those who find much reason to term Bergson the "modern Heraclitus"), yet the book as a whole is the most valuable appreciative statement of the spirit and trend of the philosophy, that the reviewer has found in a reading of much of the Bergsonian literature.

Clark University. IVY G. CAMPBELL.

BOOK NOTES

Genetics; an introduction to the study of heredity. By HERBERT EUGENE WALTER. New York, Macmillan, 1913. pp. 272.

This is by far the best summary that has appeared in English or any other language upon the subject from a biological point of view. With the aid of its 72 charts and diagrams, it presents a broad and comprehensive picture of recent studies in heredity, its carriers, variation and mutation, the inheritance of acquired characteristics, the pure line, segregation and dominance, reversion to old types and the making of new ones, blending inheritance, determination of sex, and finally the last two chapters apply these principles to man and treat human conservation. Of course many will wish the author had expanded these latter chapters more. Nevertheless it is necessary to know the basis of the work done on plants and animals and this it is the chief purpose of the author to give.

Über das Studium der Individualität, von A. LASURSKI. Pädagogische Monographien, hrsg. von E. Meumann. Band 14. Leipzig, Otto Nemnich, 1912. pp. 191.

This book, translated from the Russian into the German, which is dedicated to the memory of Dostojewsky, ends with a program for investigating personality in its relations to the environment. The author's key-word is *Neigung* or inclination, which he uses evidently in both a psychic and physical sense. The physiological basis of the soul is given abundant recognition, but this is, after all, a basis for psychological and genetic treatment. The analysis of character is made much of. This work is a résumé of many long articles which the author has published in Russian in the last five years. It is more general and philosophic than W. Stern's "Differential Psychology," but is animated by much the same ideas.

Freud's theories of the neuroses. By EDUARD HITSCHMANN. (Nervous and Mental Disease Monograph No. 17.) New York, Journal of Nervous and Mental Disease, 1913. pp. 154.

This is a very timely translation of an attempt to make a digest of Freud's views upon this subject, and is a compend which will immediately associate itself with the recent conspectus of Brill. He first treats the general theory of neuroses, then follow chapters on the true neuroses, the sex instinct, the unconscious, the dream, hysteria, obsessional neuroses, psychoanalytic method of investigation and treatment, general prophylaxis, application of psychoanalysis; then he gives a chronological review of Freudian writings and concludes with reference to publications in English.

The science of human behavior. By MAURICE PARMELEE. New York, Macmillan Co., 1913. pp. 443.

This book and the author's attitude are interesting and timely. Although not proclaiming himself an adherent of the extreme view of

Watson, he takes a position not unlike his. He begins with the physicochemical basis of behavior, then its anatomical and physiological basis, behavior of the lower animals, tropism, evolution of behavior, of the nervous system, reflex action, localization, instinct, nature of intelligence, consciousness, sensation, attention, feeling, pleasure-pain and emotion as conscious elements, personality, intelligence, consciousness and the nature of mind, beginning of social evolution, insect societies, especially ants, vertebrate societies, factors of social evolution.

Die philosophischen Auffassungen des Mitleids; eine historische-kritische Studie. Von K. ORELLI. Bonn, A. Marcus und E. Webers Verlag, 1912. pp. 219.

The writer first gives us an historical account of the conceptions of pity in ancient philosophy, the patristic, scholastic age, and in the sixteenth to the eighteenth centuries, and then treats of the English, French, Dutch, Kant and his followers, discriminating between speculative, systematic, sociological, and evolutionary viewpoints. In the second systematic part he attempts a psychological explanation. It is first as feeling and idea or a complex of ideas, its pleasure and pain attributes and its relations to self, its object, the sufferings of the pitied one, the results of it, its value as a motive and a quietive. Finally as to its ethical evaluation, he treats this from the standpoint of emotionalism, intellectualism, social altruism, individualism, egoism, and distinguishes between organists and mechanists, pessimists and optimists, passivists and activists, dogmatists, sceptics and hedonists and utilitarians, and finally discusses its share in ethics and the metaphysical treatment of it.

A contribution to a bibliography of Henri Bergson. New York, Columbia University Press, 1913. pp. 56.

This bibliography includes 90 articles and books by Professor Bergson, including translations, 417 articles and books about him in 11 different languages. The one translated into most languages is his "Introduction to Metaphysics." This bibliography was compiled by the staff of the library of Columbia under the direction of Professor Dewey, who has a short introduction.

General paresis. By EMIL KRAEPELIN. (Nervous and Mental Disease Monograph Series No. 14.) New York, Nervous and Mental Disease Publishing Co., 1913. pp. 200.

This is an authorized English translation by Dr. J. W. Moore and is one of a series of Nervous and Mental Disease Monographs. We have in English now only the authorized "Introduction to clinical psychiatry," but in the field treated in this volume there have been very important additions to our knowledge of late, although we are far from the solution of the paresis problem. The topics are general symptomatology, clinical forms, postmortem finding, etiology, nature, diagnosis, treatment.

L'Umorismo; note di estetica psicologica. Da GIUSEPPE FANCIULLI. Florence, "La Cultura Filosofica," 1913. pp. 128.

The writer first treats of the humoristical temperament, with something of its material and its psychological characteristics, then of the

artist, and finally of contemplation, including the intellectual and the motor factors.

The origin and nature of life. By BENJAMIN MOORE. New York, Henry Holt & Co., n. d. pp. 256.

The writer considers and compares physical and psychical evolution, genesis of electrons, atoms, the cosmic evolution of sense, chemical compounds of the earth, building materials for living matter, evolution of colloids, origin of life and how it came to earth, living organisms at work, waking and sleeping, fatigue and respiration. A very brief bibliography of one page is appended.

Wahrheit und Wirklichkeit. Untersuchungen zum realistichen Wahrheits-problem. Von ALOYS MÜLLER. Bonn, A. Marcus und E. Webers Verlag, 1913. pp. 64.

The author contrasts the idea and the criterion of truth, its conformity and worth theory, and after giving his general results, discusses at some length the possibility of various systems of truth and the reality character of logical laws.

The metaphysics of historical knowledge. By DEWITT H. PARKER, University of California Publications in Philosophy, Vol. 2, No. 5. pp. 103-186. Berkeley, University of California Press, 1913.

After considering the general character of historical knowledge and the nature and possibility of representative knowledge of the past, the author discusses at some length the nature of time, temporal experience, the scope and properties of time, the metaphysical status of the past, the nature of historical truth, historical verification, historical truth and insistence.

Psychologische Untersuchungen, herausgegeben von THEODOR LIPPS. II. Band, 2. u. 3. Heft; Theodor Lipps: *Zur Einfühlung.* Leipzig, Wilhelm Engelmann, 1913. pp. 491.

This is the long promised and long expected work which will be read with the greatest eagerness by those interested in the subjects the author treats. These are first of all *Einfühlung,* first for newly-made objects, second for the determination of objects; then the activity of apprehension, relations, *Einfühlung* and *Urteil,* the ego and its objects, activity, *Einfühlung* and the impression, the empirical deception of optical measurements, *Einfühlung* into free sense appearance, and finally *Einfühlung* in general. The author has indulged to the full his prejudice against any kind of index or introduction, so that the reader has no guide or compass, and, in fact, no orientation save to read the whole book itself.

Grundzüge der Ethik, mit besonderer Berücksichtigung der pädagogischen Probleme. Von ELSE WENTSCHER. Leipzig, B. G. Teubner, 1913. pp. 116.

This little introduction treats first of the derivation of ethical norms, the origin of ethical ideas and the analysis of knowledge, the eudaemonicological process of the foundation of ethics, moral good according to Socrates and Plato, Kant's imperative, psychological analy-

sis of the will activities, problem of freedom, the actualization of ethical ideas in life, and the ethical foundations of pedagogy.

The elements of psychology. By DAVID R. MAJOR. Columbus, Ohio, R. G. Adams & Co., 1913. pp. 411.

This is designed as a first book for the beginner. Instead of being a champion of structural psychology or advocating a psychology in terms of behavior, the writer has chosen a third eclectic course with no special regard to the agreement of topics and matter. He has included a brief description of the structure and function of the nervous system, and leans chiefly on the very diverse authorities of James and Titchener. Such a book as this would have been a god-send to teachers years ago, but to-day, with such a large number of elementary textbooks built upon a plan varying in but really unessential details from this, it is a little difficult to justify its existence; at least the observant reader cannot avoid thinking that the author might more profitably have spent the same time and energy in attempting to do some more special work.

A first course in philosophy. By JOHN E. RUSSELL. New York, Henry Holt & Co., 1913. pp. 302.

This book has been awaited a long time, and will greatly interest all those concerned for the subjects of which it treats. The author has been an active and able teacher of philosophy for nearly a quarter of a century, and he has tried to set forth the results of his thinking in a lucid way in order to encourage students to philosophize for themselves, and has also maintained a commendable degree of impartiality between different schools. The bibliography, too, has been carefully chosen and is admirably fitted for its purpose, and is not too copious. Part first discusses reality, its meaning, nature, the one and the many, the soul and its relation to the body and cosmology; the second, epistemology, takes up the doctrine of knowledge and its meaning, Kant, Royce, pragmatic theory of knowledge, meaning of truth, reality and the object, and finally, objections to the pragmatic theory; the third part is devoted to conduct, the problems of morality and of religion.

Aus dem Seelenleben des Kindes; eine psychoanalytische Studie. Von H. VON HUG-HELLMUTH. Leipzig, Franz Deuticke, 1913. pp. 170 (Schriften zur Angewandten Seelenkunde, hrsg. v. Sigm. Freud. 15. Heft.).

This is an attempt to describe the psychic life of the young child in terms of the Freudian psychology, laying much stress upon the various traits that Freud conceives as polymorphic perversity. The writer has done her task well, but from a very narrow standpoint. Most of her knowledge seems to be derived from Scupin's *"Bubi's erste Kindheit,"* which is a record by days and weeks of the development of a single infant. She has glanced here and there at Compayré and B. Goltz, but few others. While students of childhood will be grateful for her little epitome, it is very evident that there is a vast body of facts entirely outside her purview which bear upon the topic, a great many facts which should be treated from the psychoanalytic standpoint. It is to be hoped that we may have a new edition of the book which will attempt something of this work.

Philosophie des Möglichen. Von JOHANNES MARIA VERWEYEN. Leipzig, S. Hirzel, 1913. pp. 240.

The writer first considers the value of the possible in education to reality, then the fundamental questions that underlie it, its relations to the freedom of will, the difference between what is thinkable and what is conceivable, the *Verdinglichung* of possibility, the possible and the historic method, the possible in theology and finally in life.

A study in incidental memory. By GARRY C. MYERS.. (Archives of Psychology No. 26, February, 1913.) New York, Science Press, 1913. pp. 108.

The studies here deal with incidental memory for proportions and areas of well-known objects, incidental memory for words, letter square tests, the watch dial experiment, test of events with dates, rapid estimation of the number of letters in words, incidental memory for extent of motion.

Einführung in die Lehre vom Bau und Verrichtungen des Nervensystems. Von LUDWIG EDINGER. 2nd enl. and rev. ed. Leipzig, F. C. W. Vogel, 1912. pp. 234.

This second edition has been brought up to date and gives due recognition to the discoveries that have been made since the first edition of 1909. All in all, it is perhaps the best, the most compact, of all the compends upon this subject, with its 176 cuts.

The game of mind; a study in psychological disillusionment. By PERCY A. CAMPBELL. New York, Baker & Taylor Co., 1913. pp. 80.

The writer treats seeing, thinking, knowing, feeling, remembering, consciousness, as games in a way rather more unique than clever.

Jahrbücher der Philosophie. V. 1. Edited by MAX FRISCHEISEN-KÖHLER. Berlin, Ernst Siegfried Mittler und Sohn, 1913. pp. 384.

This work is to be a critical survey of present-day philosophy. The present volume contains eleven articles by different writers, the chief of which are the theory of knowledge and the border questions of logic, natural philosophy, the principle of relativity, the problem of time, the philosophy of organic life, the basal questions of psychology, a survey of experimental psychology in 1911 (by Messer), the philosophy of history, aesthetics.

Confession d'un incroyant; document psychologique recueilli et publié avec une introduction, par le Dr. EUGÈNE BERNARD LEROY. Paris, Émile Nourry, 1913. pp. 93.

This is an interesting and extremely frank statement covering the four periods of infancy, first communion, adolescence and maturity.

Economics as the basis of living ethics; a study in scientific social philosophy. By JOHN G. MURDOCH. Troy, N. Y., Allen Book and Printing Co., 1913. pp. 379.

The chief topics in this work are the economic interpretation of history, ethics and theories of property, ethics and productivity theories

of interest, Austrian-Yale theory of interest, interest as exploitation, economics in Kant's ethics, ethics and economic determinism.

Der Traum, psychologisch und kulturgeschichtlich betrachtet. Von RICHARD TRAUGOTT. Würzburg, Curt Kabitzsch, 1913. pp. 70.

The effect of conjugation in paramecium. By H. S. JENNINGS. And *Biparental inheritance and the question of sexuality in paramecium.* By H. S. JENNINGS and K. S. LASHLEY. Reprinted from the Journal of Experimental Zoology, Vol. 14, No. 3, April, 1913. pp. 279-446.

Memory, a contribution to experimental psychology. By HERMANN EBBINGHAUS. Translated by Henry A. Ruger and Clara E. Bussenius. New York, Teachers' College, Columbia University, 1913. pp. 123.

Bewegungslehre, Heft 14/18. VOLKMANN. Charlottenburg 4, Friedrich Huth's Verlag, n. d. pp. 95.

What is new thought? the living way. By CHARLES BRODIE PATTERSON. New York, Thomas Y. Crowell & Co., 1913. pp. 248.

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Dresden, 20. März, 1913.
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Sehr geehrte Redaktion!

Das Januarheft Ihrer Zeitschrift enthält eine Rezension meines Werkes: "Lehrbuch der Psychologie," welche mich zu einer Erwiderung notigt. Selbst seit Jahren mit der kritischen Besprechung wissenschaftlicher Werke vertraut, bin ich mir wohl bewusst, dass nicht jeder Tadel eines Buches zu einer öffentlichen Erwiderung berechtigt. Es giebt aber auch hier ein ungeschriebenes Gesetz der Gerechtigkeit und Billigkeit, bei dessen Verletzung dem Angegriffenen das Recht der Verteidigung zustehen muss und das Sie, wie ich hoffe, veranlassen wird dieses Schreiben in Ihrer Zeitschrift zu veröffentlichen. Mein Kritiker hat es sich sehr leicht gemacht. In wissenschaftlichen Kreisen ist es üblich, die Ablehnung eines ernsthaften und wertvollen Forschungswerkes nur so ernsthafter zu begründen, je schroffer sie ist. Mein Kritiker stellt sich ausserhalb dieser löblichen Sitte. Er betont statt dessen die Zugehörigkeit des Verfassers zu einer "technical school," offenbar in volliger Urkenntnis der Technischen Hochschulen Deutschlands, welche die ordentlichen Lehrstühle ihrer humanistischen Abteilungen, wie in meinem Fall, so in der Regel, mit Universitätsdozenten besetzen, und er spricht von des Verfassers Stellung zur experimentellen Psychologie, offenbar ohne die betreffenden Teile des Werkes auch nur oberflächlich gelesen zu haben. Um zu zeigen, wie andere Kritiker urteilen, führe ich den letzten Satz der ersten in einer deutschen wissenschaftlichen Zeitschrift erschienenen Rezension (Literarisches Zentralblatt 1913, Nr. 4) hieran: "Indess sind dies kleine Ausstellungen, die nicht darüber hinwegsehen lassen dürfen, dass hier ein ausgezeichnetes und für den Gelehrten wie den Lehrer in Zukunft einfach unentbehrliches standard work vorlegt.'

Ich hoffe, dass es auch vorurteilsfreie amerikanische Leser geben wird, welche sich diese in der deutschen Wissenschaft bereits anerkannte vollständigste Gesammtdarstellung der Psychologie (kein "textbook" im amerikanischen Sinne) nicht werden entgehen lassen.

In vorzüglicher Hochachtung

PROFESSOR DR. PHIL. THEODOR ELSENHANS.